A Constructive Critical Assessment of Feminist Evolutionary Psychology

by

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Author's Declaration:

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

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Abstract

In this dissertation, I develop an approach to philosophical critique of morally relevant science and apply this approach to a new branch of evolutionary psychology called "feminist evolutionary psychology." Morally relevant science is science that produces knowledge that has the potential to risk harm to humans, non-humans, or the environment. For example, a science that produces claims about women that reinforce prejudicial beliefs about women is a morally relevant science. The approach I develop, what I call the "social-dimensional approach," is designed to assess a science's epistemic and ethical dimensions which makes it ideal for the assessment of morally relevant science. My development of the social-dimensional approach is informed by an analysis of the philosophy of biology literature on the criticism of evolutionary psychology (EP), the study of the evolution of human psychology and behaviour. I apply the social-dimensional approach to feminist evolutionary psychology and show that this new science has serious epistemic and ethical flaws. I address the implications of these flaws and offer recommendations for how feminist evolutionary psychologists can amend them. I argue the social-dimensional approach has use beyond evolutionary psychology and can be used for the assessment of morally relevant science more broadly.

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Chapter 1: Introduction

Evolutionary psychology (EP), the study of the evolution of human behaviour and psychology, claims to be the science of human nature. Who humans are biologically, they say, sheds light on who we are socially. This leads many evolutionary psychologists to purport that their science is immediately relevant to how we understand and manage issues such as our well-being, morality, relationships, and even our social governance (e.g., Buss 2006; Fisher et al. 2013; Thornhill and Palmer 2000). Reinforcing their programme and their confidence in the claims they make, this research has been enthusiastically received by the public and select academic forums. Their papers have been published in prestigious psychology journals and some interdisciplinary science journals such as *Personality Processes and Individual Differences, Journal of Personality and Social Psychology, Behavioral and Brain Sciences, Human Nature, Proceedings of the National Academy of Sciences.* Their theories are talked about on radio and TV shows such as The Today Show, CNN, and NPR Talk of the Nation.[1] They are even talked about by celebrities. For example, Gweneth Paltrow cites EP research on her blog to explain and justify her separation from her husband Chris Martin.[2] In the post, Paltrow credits research by two evolutionary psychologists who theorize, because humans are living much longer now than they were in the upper Paleolithic

[1] For example, see evolutionary psychologist David Buss's interview with Matt Lauer on The Today Show about the evolutionary significance of jealousy: https://www.youtube.com/watch?v=4qsgiGbZpKo; Jennifer Pozner's (2000) coverage of the media attention evolutionary psychologists Randy Thornhill and Craig Palmer's research on rape received. Also see Reason TV's interview with evolutionary psychologists Leda Cosmides and John Tooby: https://www.youtube.com/watch?v=n_WrKno972U.

[2] See, http://www.goop.com/journal/be/conscious-uncoupling.

period, they are not evolved to be in the long-term relationships that are expected in many cultures today. Some evolutionary psychologists are even celebrities themselves, having sold thousands of books to the general public. Thornhill and Palmer's *Natural History of Rape*, for instance, sold over 10,000 copies, and an extra 10,000 copies were ordered, just in the first week of its publication (Pozner 2000). Because of its public sway and its power to influence our policies and our beliefs about people, it is important that EP be under constant scrutiny.

Philosophers of biology have stepped up to develop critiques of EP (e.g., Buller 2005; Dupré 2001; 2012; Meynell 2012; Richardson 2007). Philosophical criticisms have exposed EP's flawed use of evolutionary science and its harmful social and political implications. Feminist critics, in particular, have taken serious issue with evolutionary psychology theories on sex and sex roles (e.g., Fausto-Sterling, 1997; Lloyd, 2003; Meynell 2012; Martin, 2003). These feminist critics problematize the essentializing and biologically deterministic nature of EP theories about sex, and call EP out on the androcentrism and sexism that's evident in its topics of study, experimental designs, and data interpretations.

What is curious, then, is the recent development of a *feminist* evolutionary psychology (FEP). FEP researchers incorporate feminist values and practices into their science in order to come to grips with the sexist values that are so explicit and pervasive in traditional evolutionary psychology. In particular, they are critical of traditional sexist practices and assumptions in evolutionary psychology that lead to a hyper-focus on males and provide unwarranted support for the notions that females are passive and coy. Feminist evolutionary psychologists incorporate research practices that highlight the importance of females and women for evolutionary change.

On the face of it, this development of a feminist evolutionary psychology seems like a good thing. It appears to be evidence that evolutionary psychologists are taking feminist

concerns about androcentrism and sexism in EP seriously. This promises epistemic benefits as prejudices such as androcentrism and sexism can distort how a scientist collects and understands their data. FEP scholars' efforts to do away with such prejudices in EP can enhance their ability to produce knowledge that accurately depicts sex and gender. What is more, attempts from FEP scholars to introduce feminist values and practices into their science can stimulate interest in a broader set of research questions, and lead to the development of new methods and theories. For instance, FEP scholars' interest in increasing the focus on women as subjects in human evolutionary study has led to new questions about how women compete with one another over mates and resources (e.g., Fisher 2013; Liesen 2013). Feminist evolutionary psychologists' concerns about sexism in evolutionary psychology theories and methods has also required that FEP scholars develop new theories and methods that can more impartially conceptualize and measure evolutionary phenomena (e.g., Gowaty 2013; Heywood 2013). This kind of improvement in the scope and quality of our knowledge about women could have social benefits as it might better inform how we approach the complexity of women's issues.

It is, however, important to carefully assess FEP for several reasons. First, because feminist evolutionary psychologists still conduct their research broadly within the disciplinary framework of EP, their epistemic practices (e.g., theories, assumptions, data practices) may be flawed in similar ways that EP practices are. Second, because feminist evolutionary psychologists still operate within an EP framework, the sexism embedded in EP might also be present in FEP. In EP research, men are often attributed qualities and roles that pertain to leadership and selfishness, and women qualities and roles that pertain to servitude and nurturance. These qualities are often claimed to be innate and hard to change. For example, evolutionary psychologist Robert Wright once theorized that because men evolved to be more

competitive than women, "women will never break through the glass ceiling because, biologically, they have less of men's innate ambition and willingness to take the risks necessary for success" (Fausto Sterling et al. 1997, 404). Even though FEP purports to be feminist, if FEP scholars' research is guided by canonical assumptions about sex differences in EP, their theories and claims about the nature of men and women might reproduce the kind of sexism found in non-feminist EP. Third, if it turns out that FEP scholarship produces false sexist claims about women, then this also happens under the title of "feminism." This is problematic as the title could play a role in justifying the sexism in FEP research even more so than it otherwise would be in non-feminist EP research. For example, a theory that claims women's natural role is to nurture might already play into some people's pre-existing intuitions about women. But if this theory was also then interpreted as *good for women* somehow because it is "feminist," then people might also feel good about believing the theory. There is much psychological literature on how feelings and emotions affect our belief systems (see especially the collection of essays in Frijda et al. 2000). Finally, FEP has the potential to produce science that is both socially progressive and epistemically rigorous, assuming that whatever epistemic and ethical weaknesses it may have can be addressed. FEP scholars come from a wide range of disciplines and so bring new perspectives, theories, and methods to the evolutionary study of humans in general and women in particular.. They demonstrate a consciousness of some past critiques of EP and make efforts to address them. Their efforts to conduct evolutionary research on women from a feminist perspective also shows attention to the ethical dimensions of their discipline. A critical assessment of FEP can therefore help further its development.

Chapter Overview

My assessment of FEP in this dissertation is divided into five chapters (Chapter 2 – Chapter 6). Chapter 2 is a descriptive chapter about FEP. I lay out its history, feminist disciplinary aims, and theories, as well I identify the literature sample of FEP scholarship that I will assess in later chapters. In Chapter 3, I develop the methodological approach, what I call the "social-dimensional approach," that I need to assess FEP. This chapter is an adaptation of Weaver (forthcoming). My development of the social-dimensional approach is situated in the philosophical literature on socially relevant philosophy of science. Scholarship in this domain argues that philosophers of science are opportunely situated within scientific discourse to offer valuable deconstructions of and guidance in the production of scientific knowledge so as to promote socially responsible science. Informed by this argument, I develop an approach to scientific critique that is designed to assess a science's epistemic and social/moral dimensions. The development of this approach is based on my analysis of the philosophy of biology literature on the criticism of EP. From this literature, I categorize two different methods of scientific critique. The first I call the "truth-detectional" approach. Those who take this approach are first and foremost concerned about the truth of EP claims as that truth can be determined by evidence. The second I call the "social-dimensional" approach. Those who take this approach talk about the production and truth of EP claims but within a social framework. On this account, the legitimacy and perceived legitimacy of EP claims are not separate from the institutional and social processes and values that lend to their production. I argue that the truth-detectional approach risks harms to society and to the philosophy of science, but that the social-dimensional approach avoids these harms. I therefore commit to using the social-dimensional approach for my assessment of FEP.

In Chapters 4, 5, and 6 I apply the social-dimensional approach from Chapter 3 to FEP. In Chapter 4, I show that FEP remains hobbled with its use of a thoroughly criticized problematic theoretical paradigm that is commonly used in EP. I recommend alternative, more successful theoretical platforms for evolutionary study that FEP scholars can use instead. In Chapter 5, I argue that FEP scholars offer the wrong data, or do not offer enough of the right kinds of data to support the evolutionary hypotheses they advance (i.e., adaptationist hypotheses). To make my assessments, I develop a category of critique, what I call the operationalization of evolutionary phenomena (OEP) check. This new category concerns the success of feminist evolutionary psychologists' attempts to operationalize evolutionary terms in the context of studying humans. I develop and apply this critique in the first section of this chapter. I go on to show how past and well-known criticisms of EP data practices remain relevant to FEP. This reproduction of errors in FEP data practices is concerning, I point out, as the mistakes are fundamental errors that evolutionary biologists and philosophers have cautioned against since the 1970s and 1980s. I offer recommendations for how FEP scholars might address the weaknesses in their data practices.

In Chapter 6, I assess the role of social values in FEP and FEP's social impact. I demonstrate that harmful and distorting social values remain part of FEP knowledge claims and risk harm to especially women. I reveal how these social values guide FEP research in terms of how FEP scholars understand the relationship between feminism and evolutionary psychology, their topics of study, and how they interpret data. I argue that these values (1) compromise the quality of FEP research, and (2) jeopardize FEP's feminist aims. I provide recommendations for how FEP scholars might better scrutinize their values so as to avoid the influence of harmful and distorting values.

In the conclusion, I summarize the work I present in this dissertation and make a case for the broader use of the social-dimensional approach.

Chapter 2: The Literature Under Investigation, Feminist Evolutionary Psychology

The purpose of this chapter is to provide a descriptive overview of feminist evolutionary psychology (FEP). In this chapter, I lay out FEP's history, feminist disciplinary aims, theories, and identify the sample of FEP research that I will be conducting my analysis on. It is important that I note here at the outset that I will not be conducting any sort of assessment of FEP in this chapter. I intend my presentation of FEP scholarship to reflect only the views of its practitioners. I do not intend to be either laudatory or critical of it. Many of the theories and studies I present in this chapter will be critiqued later in this dissertation.

2.1. Integrating Feminism with Evolutionary Psychology: A Brief History

Feminism and evolutionary theory have been crossing paths since even as early as Darwin's own feminist contemporaries, Antoinette Brown Blackwell (1875) and Clémence Royer (1862). The relationship between feminism and evolution has long been antagonistic. Feminists have been critical of evolutionary theories about human behaviour that are based on unjustified deterministic assumptions, or are essentialist or sexist in nature. And some evolutionary researchers have, in turn, been critical of feminists for overemphasizing cultural or environmental causes of human behaviour, or for mixing "ideology" with science. A second, less talked about relationship between feminism and evolution is the union of feminism and evolutionary science. While this second relationship is also old, feminists working *as* evolutionary biologists, for example, have been around since at least the 1970s (e.g., Altmann 1974; Haraway 1978a; 1978b), and feminists have even talked about developing a *feminist* evolutionary biology (e.g., Fausto-Sterling, Gowaty and Zuk 1997; Smuts 1995). Interestingly,

conversations about incorporating feminism with evolutionary research have surfaced in evolutionary psychology. This is surprising as evolutionary psychologists and feminists have been especially vocal about their disagreements (Gannon 2002).

Early attempts to integrate evolutionary psychology and feminism are found in David Buss and Neil Malamuth's (1996) edited volume, *Sex, power, conflict: Evolutionary and feminist perspectives*. Scholars in this volume grapple with the cross-fertilization between feminism and evolutionary psychology which, they purport, is accomplished in three different ways in the volume (Buss and Malamuth 1996, 5). Barbara Smuts (1996) combines EP and feminism by applying EP perspective to a feminist issue (i.e., male aggression). Neil Malamuth (1996) and Felicia Pratto (1996), on the other hand, apply both EP and feminist insights to a single topic, i.e., sexual aggression and sexual politics, respectively. Finally, David Buss (1996) offers a metatheory for how evolutionary psychology and feminism can be integrated more generally.

The 10 years following *Sex, Power and Conflict* saw a few more attempts to integrate feminism and evolutionary psychology (Campbell 1999; 2006; Peters et al. 2002; Vandermassen 2005). Beginning her work around this time, Anne Campbell has done extensive research on the evolution of female aggression. Her work shows that women use aggression, as do men, as a means to compete for reproductive and survival advantages. For a long time, much evolutionary theory on humans assumed that women predominantly had a "passive" role in evolution. Scientists assumed that the variation and competition associated with evolution by natural selection was primarily associated with males rather than females. Work such as Campbell's, that shows how women compete and create sites of selection, is meant to undermine this assumption.

Griet Vandermassen's (2005) book, *Who's Afraid of Charles Darwin: Debating Feminism and Evolutionary Theory*, is a thorough extension of David Buss's (1996) call for a metatheory for feminism and evolutionary psychology. Both Vandermassen (2005) and Buss (1996) are critical of the feminist scholarship that has either ignored or critiqued evolutionary psychological accounts of sex and social issues pertaining to sex. They argue that feminists are mistaken to conclude that evolutionary psychology is irrelevant or antithetical to feminist ends. They demonstrate how various evolutionary psychological accounts of sex can augment feminist scholarship. For one, they point out how EP accounts can offer insights into the nature of men and women that have liberatory implications. For example, Vandermassen and Buss both discuss how evolutionary psychological accounts of women's sexuality counter patriarchal notions that women are naturally "coy." One popular EP account they highlight (i.e., Buss and Schmitt 1993) predicts that it sometimes benefits women to have a promiscuous mating strategy. Such benefits might include immediate extraction of resources, using short-term mating to evaluate long-term prospects, and gaining increased protection.

Second, Buss and Vandermassen suggest that evolutionary psychology can help feminism by providing a more complete account of the phenomena feminists study. In particular, they draw attention to the distinction between ultimate or evolutionary causes of behaviour and immediate or proximate causes. Ultimate causes, in EP research, are attributed to natural selection, and they are said to answer overarching "why" questions (e.g., why are most societies patriarchal?). Proximate causes in EP, on the other hand, are said to answer “how” questions (e.g., how can patriarchies be subverted?). Proximate causes are psychological, physiological, or environmental, and they are the more immediate causes for phenomena. Buss (1996) and Vandermassen (2005) suggest that evolutionary psychology and feminism, respectively, specialize in explaining

behaviour from each of these two levels of causality. They point out that the feminist focus on cultural explanations for phenomena is essentially a type of proximate explanation. For example, the explanation that sexual coercion is motivated by male dominance and control is an explanation that taps into a psychological motivation, which is a proximate cause. On the other hand, because evolutionary psychology is more interested in answering overarching why questions, the evolutionary psychological account that sexual coercion is a manifestation of male-male competition for mates is intended to explain *why* men have the psychological motivations that they do for sexual coercion. In this sense, Buss and Vandermassen argue that evolutionary psychology and feminism, together, can offer a more complete account of the phenomena the two areas are mutually interested in.

Discussions on and research embodying the integration of feminism and evolutionary psychology have surfaced more strongly over the last five years. A special issue in *Sex Roles* in 2011 was dedicated to exploring whether feminism and evolutionary psychology are allies or adversaries. Work in this volume includes (1) scholarship of feminists who are critical of evolutionary psychology, (2) scholarship by evolutionary psychologists who argue in favor of integrating feminism and evolutionary psychology, and (3) scholarship by evolutionary psychologists that, without explicit discussion of the debate, approaches feminist-relevant issues from an evolutionary psychological perspective. In 2009 through 2011 Maryanne Fisher, Rose Sokol-Chang, and Sarah Strout formed the Feminist Evolutionary Psychology Society (FEPS) (Sokol-Chang and Fisher 2013). This society's mission is to promote and bring together feminist-orientated EP research. Since the formation of this society, FEPS has held numerous conferences and a number of its members have published an edited volume, *Evolution's Empress: Darwinian*

Perspectives on the Nature of Women (see Fisher et al. 2013), which is meant to describe and present representative FEPS work (Sokol-Chang and Fisher 2013).

The advent of FEPS marks the establishment of an official branch of evolutionary psychology that brings into practice past discussions that encouraged the integration of feminism and EP. Researchers who align themselves with FEPS follow Anne Campbell's lead by explaining how women are active evolutionary agents. Similar to Malamuth's (1996) and Pratto's (1996) work, FEPS researchers also frequently combine feminist and EP insights on a single topic. And, finally, some FEPS scholars follow in Buss (1996) and Vandermassen's (2005) footsteps and continue to develop metatheories for integrating feminism and evolutionary psychology.

2.2 Research Sample for My Assessment of FEP

It is challenging to determine the boundaries of a field, particularly a new one. For the purposes of this dissertation, the formation of the FEP Society is important as it is the self-recognition of a group of scholars as doing a particular sort of research. So, the scholarship that I analyze is all produced after that event. As well, in order to work from a sample of research that is clearly FEP, I draw my analysis from sources that have made the integration of feminism and EP explicit. A sample of FEP scholarship that both follows the formation of FEPS and makes the integration of feminism with evolutionary psychology explicit are the following papers: all 22 papers in the (2013) volume *Evolution's Empress: Darwinian Perspectives on the Nature of Women*, and a specific collection of works from the 2011 issue in *Sex Roles*. The papers from *Sex Roles* that I am including in my analysis are the papers that belong to the second and third topics of the issue, (2) scholarship by evolutionary psychologists who argue in favor of

integrating feminism and evolutionary psychology, and (3) scholarship by evolutionary psychologists that, without explicit discussion of the debate, approaches feminist-relevant issues from an evolutionary psychological perspective. These papers include: (1) works by Buss and Schmidt (2011) and Eagly and Wood (2011) who provide metatheoretical discussions for the integration of feminism and EP; (2) papers by Ellis (2011), Frisby et al. (2011), and Sylwester and Pawlowsky (2011) who apply EP theory to feminist issues; and (3) papers by Singh and Singh (2011) and Vandermassen (2011) who combine feminist and EP insights on a single topic. Importantly, all of the papers just mentioned also purport to study women as active evolutionary agents. From here on, unless otherwise stated, the works from these two sources are the works I mean when I talk about "FEP" research.

2.3 FEP's Feminist Disciplinary Aims

This next section spells out in more detail how the FEP scholars in my sample conceive of their work as feminist. I discuss two feminist aims purported by these FEP scholars.

Aim 1: As stated by members of the Feminist Evolutionary Psychology Society (see FEPS 2016; Sokol-Chang and Fisher 2013), FEP aims to study how females and women are active evolutionary agents. This refers to the study of how female variation has shaped the character, or major parts of the character, of a species. Areas of research in this regard include, though are not limited to: sex roles, competition, cooperation, mothering, parenting, health, reproduction, mating, and communication. For each of these areas, researchers highlight how the behaviours, preferences, tendencies, etc. of females create sites of selection. For example, Kathryn Coe and Craig Palmer (2013), in their research, try to show how the evolutionary advantages that cultural traditions afford are due to mothers' and grandmothers' tendencies to

pass on skills (e.g., cooking) to younger women. Studying how females and women are active evolutionary agents is meant to correct for male bias in EP. Traditionally, EP has been very focused on males and how selection acts on their phenotypes. Females in EP have historically been ignored (Hrdy 2013), downplayed as agents of evolutionary change (Fisher et al. 2103), or given credit for evolutionary change in only a few select areas (e.g., their choices of mates) (Sokol-Change and Fisher 2013).

Aim 2: to study women from an evolutionary psychology perspective in order to (i) better understand women and women's issues like gender inequality, and (ii) offer solutions to these issues. In terms of the first of these, consider two examples of how FEP scholars imagine this working. Nancy Easterlin (2013) criticizes the feminist "dogmatic insistence on constructionism, the perspective that [sex] differences are socially produced rather than the result of innate predispositions" (391). Easterlin thinks that, when it comes to the study of gender inequality, understanding sex differences from an evolutionary perspective provides a more accurate picture of how differences between the sexes promote inequality and why such inequality is harmful. By providing an analysis of a classic mimetic (i.e., "realistic") piece of fiction literature, Charlotte Brontë's *Jane Eyre*, Easterlin teases out a snapshot of male-female power dynamics in 19th century British patriarchal society. Then, by applying an evolutionary psychological theory on sex-differential mating strategies to the story, she purports to show how men "typically seek to control women as a part of a psychological profile derived from their normative reproductive strategy,"[3] and that this is harmful to women because it compromises women's evolved desire for

[3] By "normative reproductive strategy," Easterlin seems to mean *adaptive* reproductive strategy.

autonomous individuality.[4] Thus, she says, EP theory on evolved sex differences can help illuminate why certain sex differences promote inequality by pinpointing the sex-specific motivations and behaviours that contribute to patriarchal social organization.

Also consider how Julie Seaman (2013) draws on an evolutionary link between "power" and ornamentation in the animal kingdom in order better understand the harms of sexist dress codes in the workplace. Seaman points out how the most elaborately "dressed" sex (e.g., the most colorful plumage) in non-human animals is often the sex with the least amount of "power," where power specifically refers to choice in mating. This observation is based on sexual selection theory which holds that elaborate plumage, colouring, weaponry, or other kinds of "dress". in males is usually the result of female choice. That is, because females, can choose freely with whom to mate,, the "dress" of the male will become honed to her preferences. Hence, according to Seaman, the female in this scenario holds the power since it is she who has control—has all the choice—in the mating arena.

[4] She develops this latter part of her argument in the following passage: "Like much simpler organisms, through control over ourselves as physical entities we seek to negotiate our survival and reproduction, but as human beings, we do so through a complex sense of autobiographical selfhood and evolved culture (Damasio, 1999; Donald, 1991). If simple organisms require only a sense of organismic integrity as a fundamental point of reference, then humans need a far more elaborate sense of self to facilitate prospective planning and retrospective memory through the agency of narrative thought. All of these integral functions evolved within men and women and promoted bonding mechanisms within the kinship group in the ancestral past; over the long term, they contributed to the development of a profound sense of autonomous individuality, out of which all human beings, women and men, provide order, control, and meaning over their lives" (394). Men's proprietary attitudes and behaviours towards women are therefore harmful, according to Easterlin, because they thwart women's evolved desires to establish their own control and meaning over their lives.

Seaman suggests that this observation in sexual selection theory can inform how we understand and deal with the power dynamics between the sexes in the human world. She notes that a similar phenomenon exists in humans, but that instead of it being the females who are the powerful ones, it is the males. Women are the more elaborately dressed, and are so precisely because men have the power to demand it of them. She explains how requiring women to be more elaborately dressed constrains their choices and has consequences for their reproduction and fitness:

> When such constraints take place in an arena of resource acquisition—for example, in the form of workplace rules that enforce social gender norms—these consequences may be especially pernicious. Moreover, female within-sex competition often takes the form of competition over resources rather than competition for males, which suggests women are particularly susceptible to negative gender stereotyping surrounding dress...And competition in the workplace may also take the form of between-sex competition, whereby constraints on female dress are likely to put women at a disadvantage relative to men where the dress code exacts financial, time, or energy costs. (419)

She concludes that, by viewing cultural dress through this evolutionary lens, it is clear that "enforcing cultural dress norms, particularly those that involve color, decoration, status, and skin, on men and women in the workplace reifies gender stereotypes, decreases female agency and autonomy, and amounts to unlawful sex discrimination" (419).

FEP scholars also use their research as a way to develop solutions to the social and political issues women face. For example, Devendra Singh and Dorian Singh (2011) promote an evolutionary account of female body fat distribution as a way to contribute to issues surrounding women's body image. The account (developed by Devendra Singh 1993) suggests that a low

waist-to-hip ratio (WHR) of fat distribution on women's bodies (i.e., <0.8) is non-arbitrarily attractive to men because it is also an indicator of health and fertility. Throughout the paper Singh and Singh present cross-cultural findings that demonstrate how changes in male judgments of female attractiveness track changes in the size of women's WHR such that men tend to prefer a ratio lower than 0.8. They also present empirical evidence which suggests WHR is predictive of risks for major diseases, an optimal hormonal profile, and reproductive capacities. They suggest that knowledge of this evolutionary account of female attractiveness can be "empowering" to women because it promotes a normative body image that is healthy, unlike the hard-to-obtain thin ideal often portrayed in the media. On this they reason,

> If women were made aware of the link between health and beauty, such awareness could potentially minimize the oppressive search for becoming beautiful. Instead of seeing beauty as a tool of oppression, understanding how health and beauty overlap can, in fact, be empowering to women. Additionally, since mass media does not promote the idea that health equals beauty, this evidence may help to reduce the power of the media and its ability to incite body weight and shape dissatisfaction in women. (729)

2.4 FEP Theoretical Foundations

As a subdiscipline of EP, FEP researchers make use of theories that are commonly used in EP; but as a *feminist* subdiscipline, FEP researchers are also conscious that some traditional theories in EP are laden with false and sexist assumptions about females. Because of this, FEP scholars also make efforts to use or develop theories that are free of these assumptions. I will discuss, in turn, examples of theoretical foundations in traditional EP that are used in FEP scholarship, and alternative, feminist theories that FEP scholars develop or use.

2.4.1 Theories FEP Shares with EP

In this section, I will briefly discuss three minor but common aspects of the theoretical foundations of FEP that are borrowed from traditional EP, then I will move on to discuss in greater detail the prominent theoretical paradigm that plays a significant role in FEP theorizing about human sex differences.

As is the case in EP, FEP research is almost exclusively focused on evolution by natural and sexual selection, so a focus on adaptations is central to much FEP investigations. That is, generally speaking, when FEP scholars talk about "evolution" or taking an "evolutionary perspective," they mean specifically evolution by natural/sexual selection, and the study of adaptations, respectively. Broadly, this tendency to be exclusively focused on adaptations has been referred to as "adaptationism" (Gould and Lewontin 1979). Like EP, FEP research also considers the human "environment of evolutionary adaptedness" to be the Pleistocene (the geological epoch that spanned about 2,588,000 to 11,700 years ago). This is the time period FEP and EP scholars assume most human-specific adaptations evolved. As such, FEP scholars do not discuss any possible ongoing adaptations. For their purposes, the adaptive traits we see in humans today are the result of selection on human phenotypes during the Pleistocene.

Another theory FEP scholars share with EP is "massive modularity." This is the theory that the mind is made up of genetically programmed, domain-specific computational modules that were selected for during the Pleistocene to solve specific evolutionary problems of the past. This theory is supposed to explain how behaviours, preferences, tendencies, and so on can be encoded in the genes such that they can be selected for. While massive modularity isn't referred to explicitly in the FEP literature I am assessing, its principles underlie much FEP reasoning

about the mechanisms of the evolution of behaviour. For instance, evolved behaviours in FEP are often characterized to be caused by specific "innate mechanisms" in the mind that evolved to solve certain problems. This is essentially to assume massive modularity (Downes 2014).

Many FEP theories on sex differences are grounded in a particular theoretical paradigm, also commonly used in EP, that's been attributed to work by Angus Bateman, Geoff Parker, and Robert Trivers (Gowaty 2013). I will refer to this paradigm as the Bateman-Trivers-Parker paradigm (BTP paradigm) throughout this thesis. This paradigm has come under fire from feminist and non-feminist scholars for its flawed and sexist assumptions and ill success in some non-human animal research. I will talk more about these criticisms in Chapter 4. For now, I will just lay out the paradigm's tenets and describe how it is used in FEP.

The BTP paradigm is used mainly to explain the origins of sex differences and the nature and persistence of these differences in sexually reproducing species. There are three parts to this paradigm. The first part of the paradigm starts with our primordial ancestors. In their respective contributions to the paradigm, Bateman, Parker, and Trivers all include some reference to the evolution of anisogamy, which is a theoretical account of the evolution of differently sized gametes. This account looks something like the following:

> The ancestral state in eukaryotes is likely to have been a unicellular organism with isogamy, i.e. where the fusing gametes are of similar size, and hence contribute equally to the zygote (Fig. 7.1). However, it is clear that under many conditions isogamy is unstable... and in such cases soon after the evolution of gametes and sexual re-combination, selection is likely to have favoured a drive for anisogamy from the ancestral isogamy. (As described in Parker and Pizzari 2015, 137-138)

This move from isogamy (similar-sized gametes) to anisogamy (large and small gametes) is assumed to result from the possibility that more total "fusings" between gametes within the population could take place if there were more mobile micro-gametes ("males") that could fuse with the few immobile macro-gametes ("females"). Thus, a drive toward a population with two types of gametes (mobile small and immobile large) was highly likely.

The early origins of anisogamy is central to Angus Bateman's famous principle of sexual selection which can be stated: *due to the intensity of male-male competition over the insemination of females, there is higher reproductive variance among males.* Through a series of experiments that went on to become some of the most well known and highly cited experiments in the sexual selection literature, Bateman (1948) attempted to explain the alleged "general law" (first articulated by Darwin 1871), that males are eager and indiscriminating, and females coy and discriminating (352) when it comes to mating. His observations of *Drosphila* led him to report that, after one generation, the number of progeny males could produce was more variable than the number of progeny females could produce. By this he meant, whereas the number of offspring females could produce was relatively similar across females, there were some males who significantly out-reproduced other males. To explain this intensity of intra-male selection, he noted that male-male competition was more dependent on frequency of insemination. He therefore marks a distinction between how male versus female *Drosophila* can enhance their fertility. For females, maximizing their fertility is limited by the costs of egg production, whereas for males their fertility is limited by the opportunities available to inseminate females. As a result, maximal female fertility (during any one reproductive period) ends at a single batch of eggs, but maximal fertility for males can extend beyond a single batch of eggs since there is the potential to inseminate more than one female. This difference between male and female

reproductive potential, said Batman, is what explains the differences in male and female mating behaviour. Because he thought number of mates and matings is linked to number of inseminations, he reasoned it is beneficial for males to maximize the number of mates they have. For females, however, he reasoned that number of matings and mates is only optimal until their eggs are fertilized. Females, then, according to Bateman, are better off to focus less on quantity of partners and more on partner quality (e.g., partners' fitness enhancing traits). And so, Bateman develops the idea of the ardent promiscuous male, and the passive discriminating female.

Bateman of course saw this distinction between the relative reproductive potentials of male and female fruit flies (and their respective behaviours) to reflect a more universal sex difference. For this he refers to the origins of anisogamy:

> The primary feature of sexual reproduction is to be sure the fusion of gametes irrespective of their relative size, but the specialisation into large immobile gametes and small mobile gametes produced in great excess (the primary sex differences), was a very early evolutionary step. One would therefore expect to find in all but a few very primitive organisms, and those in which monogamy combined with a sex ratio of unity eliminated all intra-sexual selection, that males would show greater intra-sexual selection than females. This would explain why in unisexual organisms there is nearly always a combination of an undiscriminating eagerness in the males and a discriminating passivity in the females. Even in derived monogamous species (e.g. man) this sex difference might be expected to persists as a relic.

Bateman's data and theoretical work inform the next part of the paradigm. More than twenty years later, Robert Trivers (1972) reformulated Bateman's argument regarding the

limitations of male and female reproductive success (i.e., cost of sperm and eggs, respectively) to develop his Parental Investment Theory. Trivers (1972) defines parental investment as, "any investment by the parent in an individual offspring that increases the offspring's chance of surviving (and hence reproductive success) at the cost of the parent's ability to invest in other offspring" (55). Such kinds of effort would include investments in sex cells, feeding, and guarding, but would not include mating efforts such as searching for a mate or competing with a member of the same sex for a mate. For Trivers, understanding relative parental investment between the sexes in a species is key to understanding how sexual selection will operate in that species. He reasons,

> the sex whose typical parental investment is greater than that of the opposite sex will become a limiting resource for that sex. Individuals of the sex investing less will compete among themselves to breed with members of the sex investing more, since an individual of the former can increase its reproductive success by investing successively in the offspring of several members of the limiting sex... The potential for sexual competition in the sex investing less can be measured by calculating the ratio of the number of offspring that sex optimally produces...to the number of offspring the limiting sex optimally produces. (1972, 57)

Citing Bateman, Trivers suggests that the least investing sex will be the male, save for a few rare exceptions (e.g., seahorses). In his paper, Bateman (1948) noted the costliness of the energy investment required for female gametes versus that for males'. He says egg production "causes a severe strain on [female] nutrition" (364). Likewise, in mammals, females are burdened with "uterine nutrition and milk production" (364). Males, however, he says are not burdened by these high energy investments, but merely by the cost of sperm, which he says is cheap. This initial

difference in energy expenditure, according to Trivers, is what makes females, generally, the "limiting resource" for males.

Trivers goes on to make a number of predictions regarding how sexual selection should act on the sexes given differential parental investment. In particular, he predicts an evolutionary arms race between the sexes where each is expected to evolve adaptations and counteradaptations in their efforts to maximize their conflicting fitness goals. For males, he expects they will develop adaptations that enhance promiscuity (e.g., forced copulation, desertion) and counteradaptations to prevent cuckoldry since females might try to entice males to invest in parental care, meanwhile choosing a male of higher genetic quality to do the actual fertilizing. For females, he expects them to develop adaptations to be able to detect genetically high-quality males that will invest, and counteradaptations to prevent desertion. This type of evolutionary conflict in the form of sexually selected adaptations and counteradaptations has since been referred to as "the battle of the sexes" by Richard Dawkins (1976), and "sexual conflict" by Geoff Parker (1979) who talks at length about the phenomenon.

Together, theories about anisogamy and those put forward by Bateman, Parker and Trivers make and promote the following assumptions about males and females:

> (a) females are the higher investing sex because of the cost of their gametes, (b) males are generally more eager to mate and more indiscriminate than females, (c) male reproductive success is more variable than that of females, (d) males gain more in reproductive success from repeated matings than do females, (e) these differences in males and females will lead to sexual conflict (adaptations/counteradaptations). (a – d from Dewsbury 2005, 831)

Applying these assumptions to the study of human sex differences is the third part of the paradigm. Theoretical work in early sociobiology and evolutionary psychology (e.g., Thornhill and Palmer 2001; Tooby and Cosmides 1995) almost invariably start with the BTP paradigm to explain alleged human sex differences. Assumptions (a), (b), and (c) are apparent, for instance, in theories that characterize men as more "promiscuous" than women (Thornhill and Palmer 2001), women as more "emotional" than men (since emotionality helps pair-bonding and nurturance) (e.g., Wilson 1975), and men as natural-born oppressors of women (e.g., Cosmides and Tooby 1995), since, through oppression, men can control women's reproduction.

It is interesting to see explicitly feminist scientists continuing to use the BTP paradigm's assumptions as a starting point for predicting or interpreting human sex differences. Consider two examples where FEP scholars have leaned on the paradigm's assumptions to either predict or interpret human mating behaviour.

Fisher (2013), a founding member of FEPS, relies on assumptions (a) and (b) to predict how women's competitive strategies reflect whether they seek a short-term or long-term relationship. To describe men's and women's basic mating strategies, Fisher lays out the following.

> Women have faced sex-specific reproductive circumstances during evolutionary history; in contrast to men, women have energetically costly gametes that are comparatively few in number [**assumption (a)**]. Once fertilization occurs, it involves a substantial investment in terms of energy and time. Due to this differential in required reproductive effort, men's optimal reproductive strategy may be to seek as many matings as possible and invest little in any resulting children, while women's optimal strategy may be to carefully seek a mate and invest heavily in any children [**assumption (b)**] (as based on

> mammalian parental investment strategies as elaborated by Trivers, 1985). It is probable that these differences in reproductive strategy have influenced methods of competition. For example, men may compete for access to fertile women, while women may compete for access to men with resources and who are willing to provide paternal care (Buss, 1989; Cashdan, 1998). (24)

Fisher then adds to this account by discussing some literature that marks a distinction between male and female long- vs. short-term mating strategies. For women, she says,

> When seeking a brief interaction, women are thought to prefer a mate with high "gene quality" given that the man will presumably not invest any resources or parental care in any resulting children (see for example, Gangstad & Simpson, 2000). (24)

And for men's long-term strategies, she cites Kenrik et al. (1993) who suggest,

> when considering women for relationships of various durations, men place more importance on female attractiveness as the expected duration increases. In other words, men have lower standards for a woman's attractiveness (which is an important element of male mate preference) when seeking a mate for a one-night stand, as opposed to steady dating or marriage. (24)

Considering men's and women's short and long-term strategies together, Fisher makes a prediction regarding the types of behaviours women can be expected to engage in when competing for men. She hypothesizes that women’s competition should track the kind of relationship they are seeking. If they are seeking a brief interaction, women will compete in ways that help them advertise their physicality, sexuality, and lack of commitment. On the other hand,

if they are seeking a long-term partner, women will compete in ways that help them advertise their parenting abilities and fidelity.

Another example of a FEP scholar making use of Parental Investment Theory in their research is Frisby et al. (2011) who study flirting behaviour in American university undergraduate students.[5] When interpreting their results, Frisby et al. (2011) make use of an extrapolation of assumption (a) that is present in Trivers's parental investment theory. Recall, Trivers's (1972) suggestion that because females are the higher investing sex, they should be selective of who they mate with since it pays for them to have a mate who will provide some investment and not abandon them. Frisby et al. (2011) make use of this assumption in parental investment theory in the following way:

> when men's flirtatious behaviour was motivated by fun, women's perceptions of their physical attractiveness decreased in comparison to initial evaluations of the men's physical attractiveness. According to [parental investment theory], women are most attracted to potential mates who have resources they can, and will, invest in partner and

[5] Frisby et al. (2011) actually test predictions about human flirting that are based on both parental investment theory *and* a psychological theory called Relational Framing Theory. RTF holds that all human social interactions are interpreted by individuals in terms of affiliation and dominance. Connecting this with parental investment theory, Frisby et al. suggest that, while it may be that individuals interpret messages through an affiliative or dominant frame, parental investment theory is "important in driving the final evaluations of individuals" (691). Their results, however, don't really demonstrate a connection between the two theories, which made trying to present their results as making the connection quite confusing. Since my aim in this section is just to give examples of how FEP scholars base predictions of human behavior on the paradigm's assumptions, I thought I would keep the discussion of Frisby et al. to their use of parental investment theory, but flag that they also make use of another theory.

offspring protection. Men who are perceived as simply flirting for fun may not be seen as sufficiently serious and available to invest these resources in a potential partner. (691)

Like Fisher (2013), then, Frisby et al. (2011) draw on the paradigm directly for the study of contemporary humans. Other FEP scholars who similarly make direct use of the paradigm's assumptions include Betzig (2013), Buss and Schmitt (2011), Ellis (2011), Frederick, Reynolds, and Fisher (2013), Moscovice (2013), Oberzaucher (2013), Pridmore-Brown (2013), Sylwester and Pawlowsky (2011), Vandermassen (2011), and Wilbur and Campbell (2013). These scholars make up 40% of my sample.

There are, however, noteworthy theories in my FEP sample that differ from those commonly used in EP. What makes these theories different is largely due to the influence of FEP's feminist aims.

2.4.2 FEP Alternative Theories about the Evolutionary Study of Sex

Patricia Gowaty (2013) and Leslie Heywood (2013) argue that traditional theoretical frameworks used in EP are structured in ways that result in a biased evolutionary study of the sexes. They offer alternative frameworks that guard against this bias.

Gowaty (2013, but also Gowaty and Hubbel 2005; 2009) lays out a theoretical starting point for studying reproduction in an evolutionary context that does not assume traditional principles (as established by Bateman) of the ardent, promiscuous, and aggressive male, and the coy, reticent, and choosey female. Gowaty contends Bateman's own experiments were flawed, and her own work presents over 100 animal studies that defy his principles. [6] In her work on sex

[6] Other reviews of research on parental investment theory (a theory built on Bateman's principles) also reveal "contentious" support across animals species (e.g., Sheldon 2002; Westneat 2003).

as a reaction norm, she says one way to abandon Bateman's principles is to switch the focus from the fixity of specific adaptations of the *sexes* to the "adaptive flexibility" of *individuals*. That is, instead of assuming that males similarly faced certain environmental challenges, and females similarly faced certain environmental challenges, one should assume that "selection acted so that all individuals sense and respond to the changing environmental and social circumstances that affect their options for reproduction and survival" (2013, 93). Thus, when one observes a female being "choosey," one should not assume she is being choosey because the size of her gametes make it so, but because she has inferred from her environment (hence the adaptive flexibility of individuals) that such behaviour is beneficial. If her environment were to change, she might very well evolve to be promiscuous. Gowaty and Hubbel's model has been supported mathematically (Hubbell and Johnson 1987 and Gowaty and Hubbell 2009). Importantly, the take home message of Gowaty's (2013) article is that a "sex-neutral science of sex-differentiated behaviour and psychology is possible" (108). She argues, if Bateman's principles are flawed, lead to messy results, and are politically biased, we ought to explore alternative, more neutral theoretical avenues.

Leslie Heywood (2013) also criticizes theoretical trends in EP that encourage male bias. Among others, such theoretical trends she criticizes include: (1) an exclusive focus on genes and anatomy as the primary determinants of behaviour. This, she says, makes behaviour seem instinctual and hard to change, and deemphasizes the role the environment and learning have on behaviour; (2) an emphasis on the idea that contemporary behaviours are adaptations that evolved to solve the different reproductive problems of human males and females. Heywood points out how this leads to a fixation on women as caretakers and mothers since the reproductive problems evolutionary psychologists assume women solved mostly had to do with

mothering and trying to keep a mate. Men, on the other hand (according to evolutionary psychologists), needed to solve problems regarding competition over mates and resources. The adaptations that are presumed to result from having to solve these problems are conducive to far more versatile and socially valuable roles.

Heywood is among an army of scholars who think these theoretical assumptions are outdated and harmfully politically charged. She encourages a switch in FEP to embrace theories from what she refers to as "the extended synthesis." This theoretical paradigm incorporates insights from neuroscience, genetics, developmental psychology, and paleoecology to show "how biology and culture are inextricable, and how human behavior...incorporates cultural developments, and the way that humans actively shape their environmental niche and hence their own evolutionary process" (456). Such a switch, she says, would allow FEP researchers to abandon talk of the "stone age mind" and all the harmful politics it brings with it.

2.4.3 Theories about Females as Active Evolutionary Agents

FEP scholars tell how, since Darwin, males have occupied centre stage in evolutionary theory as the drivers of evolutionary change. Since it is possible for some males to leave more offspring than others (because males vastly outnumber non-pregnant females), then it follows that their genetic and phenotypic variation can cause variable reproduction, and if that variation has a genetic basis, then evolution by natural selection becomes possible. If females, on the other hand, are all reproducing at capacity, then there is no differential reproduction and so even if there is genetic or phenotypic variation it cannot be subject to natural selection. Hence, females were assumed to occupy a largely passive role in evolutionary change. While such a view is explicitly rejected by many evolutionary scientists today, some FEP researchers insist the evolutionary

sciences still have a lot of catching up to do since females have simply been left out of so much evolutionary research, or have been unjustifiably presented as evolutionarily passive. FEP scientists have taken on the project to update knowledge about the ways in which differential female behaviour, cognition, and physiology across females create variance in reproductive success. Some new areas of research that FEP scholars have undertaken to develop accounts of women's active role in evolution include work on the evolutionary significance of mothering, women's aggression and competition, and the importance of female mating strategies and choice.

FEP research on the evolutionary importance of mothering is one way FEP scholars attempt to show how non-human females and women can have variance in terms of reproductive success. Because how one mothers can affect how many offspring one has or how healthy one's offspring are, differential mothering strategies can lead to differential reproductive success. FEP scholars in my sample provide examples of how differential mothering strategies have led to a variety of adaptations in women (and in humans generally) including certain forms of human communication (Oberzaucher 2013; Sokol Chang 2013) and socialization (Liesen 2013; Moscovice 2013).

FEP research on mothering also responds to what FEP researchers see as an undervaluing of mothering in the context of evolutionary research (Fisher et al. 2013). They note that the topic of mothering made a splash in the 1990s but has seen a steep decline ever since. Maryanne Fisher, Rose Sokol-Chang, and Justin Garcia (2013) see this to be disturbing given that "[h]uman survival depends on the strategies that women employ for mothering" (11). Quite a few articles in *Evolution's Empress* respond to this concern that more evolutionary research on mothering ought to be conducted. For example, Kathryn Coe and Craig Palmer (2013) suggest that mothers play an adaptive role in transmitting traditions, and Nicole Cameron and Justin Garcia (2013) use

secondary resources to show that mothers have specific, though flexible, depending on the environment, behavioural adaptations to enable the adaptive development of their children.

FEP literature on female aggression points out that while female aggression might be overall less physical in nature than men's and take different forms (e.g., women are said to take more indirect or subtle forms of aggression, such as manipulation, as opposed to more overt forms that are more typical of men, like physical aggression, see Fisher 2013), female aggression nonetheless has adaptive functions like male aggression. In particular, scholars in this area have talked about how female aggression is often used to secure mates (Fisher 2013), secure or obtain resources or status (Liesen 2013), protect offspring (Campbell 1999), and secure an offspring's social status (Betzig 2013). Indeed, such uses of aggression would appear to have direct or indirect fitness benefits.

Overlapping with research on female aggression, FEP scholarship on female competition also proposes that women like men compete for fitness enhancing purposes. Here it has been suggested that women compete for mates (Fisher 2013; Liesen 2013), social status and resources (Liesen 2013; Meredith 2013). And similar to the aggression scholarship, women are theorized to compete differently from men. Whereas men are discussed to compete overtly and often in risky ways (e.g., use of physical aggression; overt hostility, see Fisher 2013, 22-23), women are discussed to compete in indirect ways that only rarely involve physical aggression (e.g., dressing up, gossip, social manipulation, prosocial strategies that enhance their likeability/popularity). With that said, however, the FEP researchers in *Evolution's Empress* who talk about female aggression and competition are sure to cite literature which documents that although physical aggression among women is not the tactic of choice, it most certainly does occur and may also be used for fitness enhancing purposes (e.g., obtaining/securing mates, social status, or resources).

Finally, FEP work on female mating strategies and choice responds to under-informed and politically charged assumptions about female passivity in the context of mating. It used to be widely assumed that females were "prizes" in male competitions for mates, suggesting that females do not do much in the way of *choosing* who to mate with but more *accept* the champions (Wilbur and Campbell 2013). Although progress has been made in evolutionary research toward a broader understanding of particularly female choice (i.e., it is now accepted that many females don't simply "accept" the champion but also have mate preferences), some FEP researchers argue that not enough attention is paid to how female mating efforts and choice really influence evolutionary change. In this regard, *Evolution's Empress* presents a number of articles that highlight the significant impact female mating and mate choice has on the evolution of a species. Just to mention a couple of these, Wilbur and Campbell (2013) propose that women's preferences for humour in a mate have contributed to shaping men's psychology, and Frederick et al. (2013) suggest that women's various resistances to socially enforced constraints on their mate choices have shaped human sexuality. For instance, because women have mate preferences, it is necessary that men make efforts to attract them. Male jealousy is also suggested to be shaped by non-monogamous female mating strategies.

Taken together, FEP scholars have done much work to show that women and females are active evolutionary agents. Their research highlights the ways in which women and females are objects of selection. From who a female chooses to mate with, to how she mothers her children, to how or whether she can acquire resources and protection, she can do so in ways that affect her reproductive success relative to other female conspecifics. As such, FEP scholars bring to light the ways in which females, like males, have variance.

Conclusion

FEP history reaches back to the 1990s beginning with discussions about integrating feminism with evolutionary psychology, and follows these discussions until the establishment of the Feminist Evolutionary Psychology Society (FEPS), a subdiscipline in EP. There are, broadly, two feminist aims in FEP scholarship. These are (1) to study how females and women are active evolutionary agents; (2) to study women from an evolutionary perspective in order to (i) better understand women and women's issues, and (ii) offer solutions to these issues. I reported that these aims are meant to be feminist as they counter male bias in EP that privileges the study of how males contribute to evolution, and provide a richer understanding of women as a way to contribute to gender equality and the bettering of women's lives. As I discussed, these feminist projects inform much FEP theory (though FEP also retains much theory from EP, especially the BTP paradigm). FEP scholars have developed theories about the active role women play in evolution, for instance, theories about the adaptive importance of human mothering, women's aggression and competition, and women's mating efforts and choices of mates.

On the face of it, FEP's emergence as a subdiscipline seems paradoxical. Feminism and evolutionary psychology have historically been at odds with one another, with feminists taking issue with poorly supported and harmful conclusions evolutionary psychologists make about humans, and evolutionary psychologists often being up in arms about feminists bringing their "ideology" into science. It is curious indeed, then, to see a branch of evolutionary psychology emerge that has explicit political aims—political aims that seem contrary to the political messages typically associated with EP. Looking closer, however, as I will do in Chapter 6, reveals this paradox to be only surface level. By applying the social-dimensional approach to FEP, an approach to science assessment I develop in the next chapter, I demonstrate how

harmful values are embedded in many FEP theories and assumptions. I will also discuss the harmful implications that arise from the presence of these values.

Chapter 3
Methodology: The Social-Dimensional Approach

In this chapter, I develop the methodology I need to conduct a feminist philosophy of science assessment of feminist evolutionary psychology (FEP). Much of what is written in this chapter is taken from Weaver (forthcoming), the published version of this chapter.

3.1 Introduction

Feminist and non-feminist philosophers of science who use their work to aid science in developing and conducting epistemically rigorous research that is sensitive to the ethical and political needs of local and/or global publics are those who are said to do "socially relevant philosophy of science" (Fehr and Plaisance 2010, 302). There has been much discussion regarding the benefits of this kind of work (see especially essays in *Synthese 177*(3); Douglas 2009; Katikireddi and Valles 2015; Kourany 2003; 2010; Longino 2002; 2013; Tuana 2013). These discussions have pointed out that philosophers of science are opportunely situated within scientific discourse to offer valuable criticisms of and guidance in the production of scientific knowledge so as to promote socially responsible science. This benefits society as rigorous, ethically and socially-focused science is a valuable social good (Fehr and Plaisance 2010; Kourany 2010; Tuana 2010; 2013). It has benefits for science because philosophical methods and insight are well-suited to tackle the metascientific issues scientists often don't have the means or time to address (Reiss 2010; Shrader-Frechette 2010; Tuana 2010; 2013). It also benefits the philosophy of science; because science is a social process, philosophers gain a broader understanding of science when they know in more detail about its social dimensions (Gannett 2010; Kourany 2003; 2010; Richardson 2010). Moreover, because philosophers of science are

also contributing to science as a social good when they do socially relevant philosophy of science, they also promote the philosophy of science as socially valuable (e.g., Shrader-Frechette 2010). Because of these benefits there has been concerted effort on the part of especially social epistemologists and feminist philosophers of science to encourage philosophers of science to pay closer attention to how science is a social process that's historically situated and value-laden. This chapter contributes to this effort. However, rather than focusing on the epistemological and social *benefits* that come from the practice of socially relevant philosophy of science, I also emphasize the *harms* philosophers of science risk when they neglect recognizing or engaging with the social nature of the sciences they discuss. I argue that philosophers of science have an obligation to recognize and engage with the social nature of the sciences they assess if those sciences are morally relevant. Morally-relevant science is science that has the potential to risk harm to humans, non-humans, or the environment.[7] Throughout this chapter, I develop an approach to science criticism that has recognition of and concern for the social dimensions of science built into it. I offer this approach as a way for philosophers who engage with morally relevant science to avoid the harms I discuss.

My argument and the approach I develop are informed by an analysis of the philosophy of biology literature on the criticism of evolutionary psychology (EP), the study of the evolution of human psychology and behaviour. From the philosophy of biology literature, I tease out two different methods of scientific critique. The first I call the "truth-detectional" approach. Those

[7] Because the normative thrust of my argument imposes obligations on philosophers of science for how they ought to conduct their research, I limit the scope of my argument to morally-relevant science. When harm is at issue, the minimal obligations I impose are justified. However, when harm is not on the table, the obligations seem inappropriate.

who take this approach are first and foremost concerned about the truth of EP claims as that truth can be determined by evidence. The second I call the “social-dimensional” approach. Those who take this approach talk about the production and truth of EP claims but within a social framework. On this account, the legitimacy and perceived legitimacy of EP claims are not separate from the institutional and social processes and values that lend to their production. I show that the truth-detectional approach risks harms to society and to the philosophy of science, but that the social-dimensional approach avoids these harms. Tallying up the points against the truth-detectional approach, I conclude that philosophers ought to abandon approaches like truth-detectionism especially in their assessments of science that risks harm, that is, morally relevant science. I therefore commit to using the social-dimensional approach for the assessment of FEP.

3.2 The Truth-Dectectional Approach

In this section, I will outline an approach to EP criticism that I call the "truth-detectional approach." In the particular philosophical works I focus on, evolutionary psychologists are criticized for (1) conducting substandard evolutionary research, (2) harbouring problematic epistemic values, and (3) conducting research with pernicious social implications. As I will discuss, what ties these three categories of critique together is a principal concern about the truth of EP claims as that truth can be ascertained by the evidence evolutionary psychologists can and do offer. When philosophers raise truth-detectionist concerns, they are raising doubts about the objectivity of EP and so attempt to invalidate the knowledge claims evolutionary psychologists make. Directly below, I provide a brief outline of these criticisms as they have been put forward

by David Buller (2005), Robert Richardson (2007), and Stephen Jay Gould (1997a and b).[8] Following this I offer a more thoroughgoing characterization of the truth-detectional approach.

3.2.1 Substandard Evolutionary Research: Problems with Theory, Methods, and Data Analyses in EP

Theory. Buller (2005), Richardson (2007), and Gould (1997a; 1997b) have dedicated much discussion to the problematic or wrongful application of evolutionary theory in EP. In particular, these scholars take issue with the tendency among evolutionary psychologists to overemphasize natural selection as an explanation for specific human behaviours—a tendency often referred to as "adaptationism" (Gould and Lewontin 1979). Buller, Richardson, and Gould's criticisms of adaptationism are aimed mainly at the various hypotheses in EP that are grounded in the "massive modularity thesis," a theory considered by many evolutionary psychologists to be foundational to EP (e.g., Barkow, Cosmides and Tooby 1995; Buss 2008; Shackelford and Liddle 2014). According to this thesis, much of human behaviour is under the influence of highly specified organ-like modules in the mind. These modules, we are told, were shaped by natural selection during the Pleistocene when humans were faced with strenuous survival and reproductive challenges. Evolutionary psychologists who adopt the massive modularity thesis advance that, because mind modules are genetically based (and so are heritable), natural

[8] Despite Gould not being a philosopher by profession, I think these particular works of his are ideal for my analysis. Gould's criticisms of EP are nothing short of philosophical and they are familiar works in philosophy of biology. What's more, Gould has taken a more social-dimensional approach in other works (e.g., Gould 1996). This demonstrates nicely that the two approaches I discuss in this paper are *methods* and need not be identified with a particular philosopher's whole corpus of work.

selection could favour some modules and, therefore, some behaviours, over others (Buller 2005; Richardson 2007). As such, they offer an account for how behaviours can be adaptations.

Putting aside the neuroscientific plausibility that the mind can be explained in terms of "modules" (but for criticisms see Buller 2005; Sterelny 1995; 2012), Buller, Richardson, and Gould think the privileging of adaptation explanations for human behaviour (as is entailed by the massive modularity thesis) is too narrow. For one, it neglects the competing, and quite successful, explanations of behaviour that have come out of the social sciences. Many of the behaviours evolutionary psychologists posit to be adaptations have also been explained in terms of developmental and social causes—causes that cannot always be traced back to the adaptive environment in the Pleistocene. Second, undue focus on adaptation explanations blocks research into other mechanisms of evolution. Gould (1997b), for example, stresses the importance of "spandrels." A spandrel is a "byproduct" of natural selection, a trait whose current usefulness is not the direct result of natural selection (1). For instance, reading and writing are useful behaviours, but one cannot say that the neurological machinery necessary to execute them evolved specifically for them. Gould suggests that many of the behaviours evolutionary psychologists conceptualize as adaptations might in fact be spandrels.

Methods. Buller, Richardson, and Gould also take issue with the kinds of methodology many evolutionary psychologists employ to test their hypotheses. In particular, they problematize uses of comparative approaches and evolutionary functional analysis, also known as "reverse engineering."[9] Considering uses of comparative approaches in EP, evolutionary psychologists are most interested in studying the kinds of behaviours that are uniquely human,

[9] Reverse engineering is when an evolutionary psychologist "attempts to reconstruct the mind's design from an analysis of the problems the mind must have evolved to solve" (Buller 2005, 92).

behaviours that are not expressed by most species that are considered for comparisons. This, of course, limits whatever inferences evolutionary psychologists can draw from behavioural comparisons. Nevertheless, as Buller and Richardson point out, many evolutionary psychologists are incautious with regard to the certainty they claim from their comparative work and can be overly liberal concerning the species they choose (e.g., Thornhill and Palmer 2001 draw on data from scorpion flies to support their conclusions about human rape).

Looking at reverse engineering in EP, Richardson (2007) reminds us that "adaptive processes and their results do not correspond one-to-one" (59). For instance, just because the *Archaeopteryx* (a bird-like dinosaur) had feathers and bird-like feet, it does not mean that those feathers were needed for flying, or the feet needed to perch in trees. Because of this, Richardson explains it is important that reverse engineering explanations of adaptation be "supplemented and augmented in a variety of ways," including being provisioned with independent evidence and evidence that is directly historical (52).[10] These extra evidential steps, however, are often ignored by evolutionary psychologists.

Data Analysis. Buller, Richardson, and Gould also criticize evolutionary psychologists for their problematic interpretations of data. They discuss instances where they have found evolutionary psychologists to fail to report relevant complications or contradictions in their data (e.g., Buller 2005, 228-252, 370-410), and overgeneralize their conclusions (e.g., Buller 2005, 210-228; Richardson 2007, 174-183).

[10] Historical evidence in the context of EP would require information about "the sort of environmental 'problem' [human] cognitive mechanisms are responding to, the phenotypic and genotypic variation present, the structure of the relevant social groups, the gene flow between them, and other population parameters" (Richardson 2007, 84).

3.2.2 Problems with Epistemic Values

Epistemic or cognitive values are those values in a science that are thought to be truth conducive. Ultimately, epistemic values are supposed to provide objective grounds for theory choice (Longino 1996). Buller and Gould point out ways that epistemic values in EP fail to be truth conducive.

Gould criticizes evolutionary psychologists' overly simplistic understanding of evolutionary theory. A simple theory, defined in ontological terms, is one that posits few causal entities or processes (Baker 2013; Longino 1996). Simplicity has been considered to be epistemically valuable for *a priori* reasons (e.g., intrinsic rational value), naturalistic reasons (e.g., complex theories depend too much on ad hoc explanations to deal with anomalies), and reasons pertaining to probability and statistics (e.g., simpler laws have greater prior probability) (Baker 2013). Gould (1997a) criticizes evolutionary psychologists for holding an *overly* simplistic account of evolution: adaptationism. According to Gould, underlying adaptationism is the "dream" that an "enormously complex and various world" can be underpinned by a single mechanism: evolution by natural selection (3). He says this "dogmatism" hurts evolutionary research since it "threatens to compromise the true complexity, subtlety (and beauty) of evolutionary theory and the explanation of life's history" (5).

Buller (2005) discusses problems with a second epistemic value in EP work, an explanatory value he thinks evolutionary psychologists overemphasize: design. Specifically, Buller argues that the value many evolutionary psychologists place on discovering and explaining design in organisms is outdated. On this he cites Peter Godfrey-Smith (1999) who refers to the emphasis on adaptation in EP as a "theoretical vestige" of natural theology's argument from design. The argument from design was originally put forward in the early 1800s

by William Paley who proposed nature to have a creator since it has design (as cited in Buller 2005, 472). To Godfrey-Smith (1999), both Paley's natural theology and adaptationism wrongly accord the problem of design a central status in our investigations of organisms. But, as Buller adds, "There is nothing in the nature of things that mandates that we should explain complex design as being more important than," for example, nonadaptive evolution, extinction, or organismic diversity. Interests in these others have advanced evolutionary theory and are crucial for the study of life (475). An over-valuing of design, then, according to Buller, limits what evolutionary psychologists can learn about the nature of organisms.

3.2.3 Pernicious Social Implications

Gould and Richardson dedicate some discussion to the pernicious social implications that are associated with some EP claims. In different ways, both theorists suggest to their readers that the many harmful claims in EP can be dismissed since they are not backed by appropriate evidence. In his discussion, Richardson brings up Philip Kitcher's (1985) normative point that "when the negative consequences of accepting some conclusion are great, and the conclusion is itself uncertain, then we should demand higher standards of evidence before we embrace it" (Richardson 2007, 34). Taking this seriously, Richardson argues that, given the social costs of so many EP claims, [11] EP should at the very least be held to the same standards as non-human animal evolutionary biology. He goes on to conclude that given these appropriate standards, most EP claims will be found to be unsupportable, and so too their pernicious implications.

[11] On this he mentions Thornhill and Palmer's (2001) theory of rape and how some see it to justify rape and "give support to rapists" (36).

Gould (1997b) also notes a potential for social harm if some EP hypotheses are accepted. And like Richardson, he appeals to the scientific inadequacy of EP to reassure that such harms are an unlikely threat:

> If evolutionary psychologists continue to push [their program] as a central dogma, they will eventually suffer the fate of the Freudians, who also had some good insights but failed spectacularly, and with serious harm imposed upon millions of people (women, for example, who were labeled as 'frigid' when they couldn't make an impossible physiological transition from clitoral to vaginal orgasm), because they elevated a limited guide into a rigid creed that became more of an untestable and unchangeable religion than a science. (6)

Thus Richardson and Gould's concerns about harmful claims in EP hinge on a concern about the evidential status of such claims. Although Richardson acknowledges that non-truth-relevant factors (such as a concern about harm) should inform our standards for accepting EP hypotheses, he nonetheless maintains that evidence and its connection to truth should have the final say. Accordingly, this would mean that regardless of how pernicious an EP claim might be, we are required to accept it if the evidence provided passes our (albeit heightened) standards. Gould (1997a), as well, is counting on the empirical inadequacy of EP for the rejection of its pernicious claims. Like the Freudians, he says, evolutionary psychologists are pushing theories that are harmful but also "untestable" and "rigid" and so will likely fail (6).

In sum, analysis of specific works by Richardson, Buller, and Gould yields a critical approach to EP that I call the truth-detectional approach to science criticism. In these works, EP researchers are criticized for their (1) substandard scientific work with evolutionary theory, methods, and data analyses, (2) problematic epistemic values, and (3) pernicious social

implications. What ties these three categories of critique together is a concern about the truth of EP claims as it can be ascertained by the evidence evolutionary psychologists can and do offer. In these specific works, Gould, Richardson, and Buller are concerned about substandard theory, methods, and data interpretations in EP because issues in these domains compromise the evidence evolutionary psychologists offer. They are concerned about problematic epistemic values because they guide EP research in ways that compromise its potential for objectivity. Even their criticisms of the pernicious social implications in EP center on truth. As I showed, both Richardson and Gould dismiss the pernicious claims in EP, anticipating that such claims are largely not adequately supported by evidence.

3.3 The Social-Dimensional Approach

When philosophers recognize the social nature of science and assess scientific theories, methods, knowledge claims, etc. as part of a framework of social processes and values, they are engaging in what I call the social-dimensional approach to science criticism. In this section I outline criticisms of EP by John Dupré (2001; 2012), Letitia Meynell (2012), and Cheryl Brown Travis (2003a) who, in these specific works, take a social-dimensional approach. I highlight three social dimensions of EP research that these critics take into consideration in their assessments of EP: social values, dissemination, and social implications.

3.3.1 Social Values

In their critiques, Dupré, Meynell, and Travis take seriously the social values that motivate and guide much EP research. Social values as I mean them in this paper are values that reflect what is deemed socially important (e.g., economic welfare, power, equality). They may or may not be truth conducing. Taking from Dupré, Meynell, and Travis's discussions, uncovering social values

in science, and learning about the various roles they play throughout the scientific process, is important for at least three reasons. First, uncovering guiding social values can help explicate *why* a scientist or community of scientists might be conducting science in the ways that they are. Second, understanding the values in a science can help explain why a given research program might be attractive to others—if those values are widely shared for instance. Third, talking in-depth about how values in a science can lead to harmful social consequences can help show why that science is not valuable to society and so should be ignored. The two social values I will be discussing from Dupré, Meynell, and Travis's discussions I call "Social Order" and "Authority." In this section, I define these values and discuss the ways in which they have been said to motivate EP research, which is the first reason, listed above, for why uncovering social values in science is important. I will address the second and third reasons respectively in sections 3.3.2 and 3.3.3.

Social Order. To value Social Order is to value an organization of people into social roles that are conducive to a certain end state of society. One can have progressive values of Social Order such that the social roles you advocate for are conducive to an egalitarian end state. However, one can also have non-progressive or harmful values of Social Order if the social roles you advocate for promote an end state of society in which certain groups of persons are oppressed or otherwise systematically mistreated (e.g., racial or class hierarchies, patriarchy). Problems with Social Order in EP are indicated in Dupré, Meynell, and Travis's discussions about essentialism and biological determinism. Here these critics lay out the ways in which EP research harbours harmful values of Social Order when they define, reify, and ground the differences between social categories in biological causes.

To be essentialist about human categories (e.g., sex, class) is to assume that there are sets of necessary and sufficient conditions that must obtain for individuals to count as members of some category or another (Grosz 1994). Dupré, Meynell, and Travis discuss ways in which EP hypotheses are embedded with essentialist assumption about sex (but also class as I will talk about later). Evolutionary psychologists often cast human sexes as starkly dichotomous categories that have been shaped by natural selection. Men, we are told, have particular physical, mental, and behavioural characteristics that correspond non-arbitrarily to the specific problems they needed to solved in their evolutionary past. Because they needed to compete against one another for access to women, they evolved adaptations that make them fierce competitors: strength, leadership, selfishness, sexual prowess, promiscuity, desire for power. Women, as well, have characteristic behaviours, psychologies, and behavioural tendencies that map onto the adaptive problems they needed to solve, many of which were different from the problems men faced. Because women needed to raise offspring and land a mate to help raise those offspring, they evolved adaptive traits that make them good caretakers, choosy about their mates, and able to retain a male mate for long enough to raise offspring: sociability, empathy, emotionality, sexual coyness. Not coincidentally, this kind of essentialism about sex is conducive to a Social Order that requires men to be the leaders and breadwinners in society and women the caretakers. Essentialism allows for the categories of "men" and "women" to be characteristically different (e.g., men are leaders, women are caretakers) and separate (e.g., it is difficult for men to caretake and difficult for women to lead). Evidence that essentialism about sex in EP is motivated by a valuing of Social Order, and not the result of impartial observations of society, is put nicely by Meynell who explains that essentialism in EP can only be maintained by "willful ignorance" (2012, 21). She says,

> Although it is clear that behavior is variable and that [sex] dimorphism, when found, is typically moderate, [EP] must sideline variance in such a way as to mask the similarities between the sexes and the variety within the sexes. (21)

Issues surrounding essentialism about human categories are often closely linked with discussions about biological determinism. Biological determinism is a mode of explanation of causation that assigns biological factors primacy or, in some cases, totality over the causal space of a given characteristic. For example, a biologically deterministic view of mothering sees mothering first and foremost as a biological or "natural" inclination of women (Birke 1986, 13). Two important assumptions that often accompany biological determinism are the views that traits which are biologically determined are immutable (or at least very hard to change) and/or are desirable (Birke 1986). Along these lines, Dupré, Meynell, and Travis point out the ways in which many evolutionary psychologists root the essential characteristics of sex, and other categories in biological (evolutionary) causes. Such a move inevitably, either implicitly or explicitly, acts to ground the alleged characteristics of the social category as inherent to that category, making them seem permanent and expected. In her discussions about the tendency among many evolutionary psychologists to root gender stereotypes and differences in evolutionary causes, Travis (2003a) points out how such a tendency is a symptom of a desire to uphold social orderliness. She says,

> Western, occidental views of sex and sexuality are built around categorical dichotomies, where the creation and celebration of sex differences are understood to be crucial to social order. One gains the impression from this dichotomous

> view that society would pretty nearly collapse without the oderliness derived from these differences. (10)

Thus, biological determinism picks up where essentialism leaves off in regards to supporting a harmful Social Order. Where essentialism establishes the existence of social hierarchies across categories, and norms of membership within categories, biological determinism naturalizes the existence and justifies the persistence of these categories.

One reason that understanding the social motivations (i.e., Social Order) underlying essentialist assumptions in EP is important, according to Meynell, Dupré, and Travis, is because it sheds light on *why* evolutionary psychologists hold onto them despite criticism and contradictory evidence. As I will discuss in more detail, this draws an important point against the truth-detectional approach. If values are motivating a certain pattern of mistakes in a program, then it is crucial that these values be addressed because this means they are in part a cause of those mistakes. Attacking particular theories on account of their evidence is only part of the job if the researchers behind the theories have values that will continue to influence future research.

Authority. To value Authority is to value one's own or one's group's positions as positions of far-reaching epistemic influence. The scope of one's influence, however, ought to be bounded by one's resources (tools, data, methods, theory, etc.), expertise, knowledge, and professional/social position. Problems can arise if one either underestimates (wimpiness, unused expertise) or over estimates (imperialism, false confidence) the scope of their influence. Problems with Authority in EP are indicated in Dupré's (2012) concerns about monism. He explains, one way evolutionary psychologists illegitimately garner epistemic authority for their theories is by exploiting ideas of a reductionist "scientific unity" (e.g., 2012, 35). Dupré

problematizes the pervasive monistic assumption in science that explanations of the different phenomena across scientific disciplines are organized along hierarchical levels such that phenomena at higher levels can be explained by reducing them to phenomena at lower levels. So, for example, this would be to assume that neuroscience and psychology can be unified because the human behaviours psychologists study can be reduced to the neurological processes that are the subject of neuroscience. Part and parcel with assumptions of such scientific unity is that explanations at the lower levels are superior—i.e., they explain more about a given phenomena—to higher-level explanations. According to Dupré (2012), evolutionary psychologists are keen to emphasize the superiority of their discipline given where in the hierarchy of scientific explanations they take their own explanations to occupy. For example, Dupré (2001; 2012) cites Jerome Barkow, Leda Cosmides and John Tooby (1995) doing just this. Propounding a version of physical reductionism, these evolutionary psychologists emphasize the importance of scientific explanations that appeal to the internal structural properties of phenomena, as opposed to explanations that highlight context and environment. This, of course, is meant to epitomize EP (a science of genetics and "mind modules") as a hyper-scientific account of human behaviour, over and above the more traditional human behavioural disciplines that must contend with "chaotic" phenomena like culture (as cited in Dupré 2001, 73).

Dupré's discussion of the ideology of reductionistic scientific unity and how evolutionary psychologists can gain from this sheds valuable light on the ways in which problematic values of Authority permeate EP research. By assuming a reductionistic unity of science and claiming to occupy a crucial part of this unity, evolutionary psychologists enhance the epistemic authority of their discipline. This authority is then used to discredit other disciplines' theories of human behaviour (especially those disciplines that do not appeal to the

supremacy of endogenous causes) that they claim do not fit as tightly within the science unity that evolutionary psychology allegedly does.

3.3.2 Dissemination

Contrary to what many philosophers of biology might assume, EP is in fact widely received across public and academic contexts. While there are certainly many scholars across the humanities, social sciences, and biological sciences who reject much EP research, evolutionary psychologists are not without their allies. Evolutionary psychologists publish widely in prestigious journals,[12] receive large grants from some of the biggest granting agencies,[13] and are employed at the top universities in the world.

Dupré, Meynell, and Travis point out that the wide receipt of EP and the connections its scientists have are a nontrivial part of its knowledge production process. The institutional support evolutionary psychologists receive enables their research (e.g., through funding) but also promotes it if those institutional bodies are reputable, which, as I pointed out, they often are. This

[12] Psychology journals include: *Personality Processes and Individual Differences*, *Journal of Personality and Social Psychology*, *Behavioral and Brain Sciences*. Interdisciplinary journals include: *Human Nature*, *Proceedings of the National Academy of Sciences.*

[13] For example, one or more of the four largest EP labs in the United States (i.e., Evolutionary Psychology Lab, co-directed by Todd A. Shackelford & Viviana A. Weeks-Shackelford; The Buss Lab, directed by David Buss; Center for Evolutionary Psychology, co-directed by Leda Cosmides and John Tooby; Evolution and Human Behaviour Laboratory, directed by Michael McCullough) have received funding for their research from, among others, the following major agencies: National Institute of Mental Health, National Institute of Health, National Science Foundation, Hogg Foundation, Gordon P. Getty Trust, John Templeton Foundation, Air Force Office of Scientific Research, The Fetzer Institute.

support in turn contributes to the dissemination and legitimization of the knowledge EP produces. According to Meynell (2012), this recognition from reputable institutions, especially prestigious journals, can act as artificially confirming the truth of EP claims. She says, EP's high volume of publications contributes to "an appearance of significant empirical success" (18).

Dupré, Meynell, and Travis connect dissemination with social values. This connection provides a second reason why talking about social values is important for the assessment of morally relevant science. They mention how EP research that is embedded with widely held social values (particularly those of Social Order) can make EP attractive to broad audiences. Research that expresses deeply held values resonates with what people already believe about categories of people and so makes EP seem intuitively true.

3.3.3 Social Implications

Meynell, Dupré, and Travis, like Gould and Richardson in their truth-detectional approach, also voice concern about the social implications of EP. However, their approach to discussing these implications differs from Gould's and Richardson's in that they connect their discussions of implications to social values, and talk more at length about what the implications actually are and why they are harmful. For instance, in connection with their discussions about social values, Dupré (2001; 2012) and Meynell point out some dire ethical problems that arise from essentialist views of sex and class. Meynell points out that typifying members of a category implies that there is only one general type of person in that category and therefore erases the real differences between members of that category. This often epitomizes a paradigmatic "normal" person for that category and casts others as abnormal. Dupré (2001) shows that, when sets of characteristics assigned to the token members of a category are pejorative relative to the assumed characteristics

of other categories, this acts to marginalize the entire category. For example, he points out how the view that men of low socioeconomic status are violent instills an unnecessary fear of them and contributes to their unequal treatment before the law. Travis talks about some harmful implications associated with biological determinism. She explains how grounding sex differences in biological causes provides "reassurance about the natural, and therefore rightful, divisions of labor," and a reassurance about a "natural basis for unequal privilege" (2003,11).

This richer discussion about the harmful implications of EP research, and their connection to values, does two things for the assessment of EP that Gould and Richardson's criticisms cannot. For one, it does more to demonstrate the riskiness that is associated with accepting pernicious EP hypotheses (a goal that, recall, is in fact important to Gould and Richardson). The mere one sentence that Gould (1997b) and Richardson (2007, 36) each allocate to describing the implications of EP might not be enough to convince their readers that the implications of EP do indeed make accepting certain EP hypotheses risky. As I discuss below, losing a reader on this aspect of one's critique is not inconsequential if the reader has in fact determined that *rejecting* the theory is what is risky. Second, and referring to the third reason talking about social values is important for the assessment of morally relevant science, discussing in more detail the harms in science does work to devalue that science as a social good. As a practical enterprise, science is in the business of producing knowledge that can be used for human ends. Within such an enterprise, knowledge that thwarts our ends is therefore less valuable (see Kitcher 2001 for more in-depth discussion of this point). Talking at length about the harms in a science gives philosophers an opportunity to talk about the pragmatic dimensions of science and to point out to their readers that harmful science is also wasteful from a societal perspective.

In brief, the social-dimensional approach is an approach to science criticism that sees science, especially morally relevant science, as embedded in and so intimately affected by social processes and values. As such, taking this approach requires that the social dimensions of a science such as its social values, dissemination, and social implications be given serious consideration in connection with things like evidence. On this point, it bears emphasizing that the social-dimensional approach is not *just about* values, dissemination, implications or any other salient social dimension of science. By no means would taking the social-dimensional approach prohibit a philosopher from being concerned about evidence, theory, methods, or even truth. It is just that these latter aspects, to a social-dimensionalist, cannot be disconnected from social dimensions and so must be considered alongside them.

3.4 The Truth-Detectional Approach and Harm

In this section I discuss some of the ways using the truth-detectional approach can cause harm. For the purposes of this discussion, I mark a distinction between *facilitating* a harm and harming. By facilitating a harm I mean contributing in some way to a harm that has been initiated by someone or something else. One can contribute to a harm by adding to it (an active action) or by failing to try to prevent it (a passive action) when given the opportunity. For instance, consider the scenario of a sports team that is currently losing a game. A teammate on the losing side can facilitate their team losing by actively doing something like scoring in their own goal, or by passively not doing something like making no attempt to block scores from the other team. In both cases, the teammate acts in ways that help rather than hinder their team losing. Someone who harms, on the other hand, is an initiator of a harm. Harming, in this sense, can also be active or passive, but the subject must be the initiator of the potential harmful outcome. In this section I

consider the different ways using the truth-detectional approach can cause harm in these two senses. I show that because the truth-detectional approach is not equipped to address harms connected to science, using the approach can passively facilitate certain harms connected to science. I also discuss how the truth-detectional approach harms the philosophy of science in part because it promotes an impoverished understanding of the production of scientific knowledge. In contrast, the social-dimensional approach avoids these harms.

3.4.1 Social Harm

Two characteristics of Gould, Richardson, and Buller's approach to the criticism of EP risk facilitating harms to society. These characteristics are (1) a lack of discussion about EP's harms and (2) an implicit assumption that a harmful hypothesis that is shown to be false will be rejected.

First, remaining silent on or talking only minimally about the harms connected to EP, when one is in a position to do so, can passively facilitate those harms because such silence protects evolutionary psychologists from having to account for the harms they cause or risk. Criticizing a sexist programme in EP, for instance, because its theories are weak, its experiments are flawed, or its conclusions are too broad, allows the sexism itself to remain in the program unaccounted for. A seemingly unshakable image of the scientist depicts someone who is impartial, or at least profoundly limited in their passions by the rigidity of the scientific method. This image remains supported when scientists are continually assumed to not have partiality—especially partiality about social matters. When Gould, Richardson, and Buller dig up flaws in EP theory or methodology but ignore its harmful social dimensions, they lend support to the idea that while evolutionary psychologists can make technical mistakes, their work and actions are

irrelevant to or disconnected from issues of social harm. In the eyes of Gould, Richardson, and Buller's readers, this removes accountability on the part of EP for the harm it causes. Thinking about this on a larger scale, a broad application of the truth-detectional approach among science critics contributes more generally to the relaxing of science's accountability for harm. Less accountability can create a climate of incaution in science such that there is little incentive to be mindful of how scientific practice might effect social harms.

Not talking about harmful social dimensions in science passively facilitates social harms in another way. As I mentioned earlier, values can be embedded in scientific practice such that they guide the development of theories, assumptions, and methods, or guide how scientists draw their inferences. This idea that social values can be embedded in the prior developing conditions of theories, sets of assumptions, etc. reveals that they need not be specific to any one research instance but can carry forward to the development of subsequent theories, methods, and so on. For instance, if a prejudice is motivating a certain set of assumptions, critiquing that set of assumptions for reasons precluding the prejudice risks the next set of assumptions also being prejudiced. But if the prejudice is a part of the critique, like it would be in a social-dimensional critique, this gives scientific practitioners or institutions a chance to guard against it specifically in subsequent research. Philosophers of science are well situated to alert scientists to these more systemic kinds of harms.

When philosophers of science take the truth-detectional approach to their discussions about morally relevant science they can also passively facilitate harming their own readers.[14] When philosophers report on socially harmful science, they share that science, its claims, values, assumptions, implications, and so on with their readers. This makes their readers, who might

[14] Thank you very much to an anonymous reviewer for their insights on this discussion.

never read EP otherwise, vulnerable to any harmful content that might be embedded in the science being presented. A philosopher can passively facilitate harming their readers if they remain neutral about the harms they are exposing their readers to. Regardless of the philosopher's intentions, being neutral can give implicit support to the harms. To illustrate, consider how a philosopher who takes a truth-detectional approach to an EP "mating strategies" theory could facilitate this type of harm.

Evolutionary psychologists David Buss and David Schmitt (1993) predict sexual dimorphism regarding human promiscuity. Drawing on the BTP paradigm, which assumes that because men are the lower investing sex and so are more competitive with one another for access to females, Buss and Schmitt make four predictions about men. They say men: 1) should express greater desire for, or interest in, short term mates than will women, 2) should desire larger numbers of sex partners than will women, 3) should be willing to engage in sexual intercourse after less time has elapsed than will women, and 4) should relax their mate preference standards in short-term mating contexts more than will women (Buss and Schmitt 2011). Someone reading about this theory in philosophy of science could easily pick up on any one of the theory's many implications: e.g., it is more natural/normal/expected for men to be promiscuous than women; when a man expresses romantic interest in a woman, he most likely just wants to have sex with her. These implications are of course harmful because they reinforce prejudicial norms and assumptions about men's and women's sexualities. They affirm beliefs that women are sexually prudish, only wanting sex when it will get them something (e.g., intimacy, protection, financial support, a child); they also paint men as sex-obsessed and sexually unemotional.

As this train of reasoning (i.e., theory – implications – cultural beliefs) reminds us, more can happen in the mind of a reader of science (or philosophy of science) than mere understanding

of linguistic phrases. Readers bring their own assumptions, experiences, and beliefs to their interpretations and understanding of science. This is why, as discussants of science, it is important for philosophers to know their readers and to seriously consider how a piece of knowledge might resonate with them. Certainly we can't know the minds and idiosyncrasies of everyone, but when there exist widespread prejudices, we can expect that many of our readers will either have them or will be susceptible to them given the right "information." Buss and Schmitt's theory counts as just this kind of information. It mimics the kind of knowledge that is necessary to justify the prejudices I mentioned above. Philosophers who disseminate this kind of information to their readers are in a privileged position. They have the option to name and try to mitigate the harms the information can cause, or to remain neutral. If philosophers use their privileged position to remain neutral, they passively facilitate the harm the information can cause.

Building on this, trying to mitigate harms in science by simply showing that the science is "untrue" is unlikely to be effective for a couple of reasons. First, philosophers have to assume that not all of their readers will be on board with their criticisms of science. When this happens in a context where a philosopher is presenting research with prejudicial implications, the philosopher succeeds at nothing other than exposing their reader to a theory that gives them reason (or more reason) to hold a prejudice. Second, even if a philosopher succeeds in persuading their reader that a certain theory is flawed, theories that speak to culturally sensitive subject matter could be hard for readers to reject if rejecting them seems personally risky. For instance, readers of philosophy that presents Buss and Schmitt's theory might wager that it is better to believe the theory and apply it to their own lives, despite the flaws the philosophers talk about, than risk having an "abnormal" sexuality.

3.4.2 Harm to the Philosophy of Science

The truth-detectional approach, as it is applied by Gould, Richardson, and Buller in the specific works discussed, can also harm the philosophy of science. For one, these theorists' use of the truth-detectional approach assumes an impoverished understanding of the production of scientific knowledge. This in turn leads to lower quality philosophy of science research than one could otherwise offer with a richer understanding of science. Much philosophical scholarship over the last decades has moved beyond the idea that science is asocial. Lessons from especially social epistemology and feminism have taught convincingly that few if any aspects of the scientific process are free of social dimensions like social values (Bluhm 2013a; 2013b; Douglas 2009; Dupré 2012; Haraway 1989; Keller 1985; Kitcher 1985; Kourany 2010; Longino 1990; 2002; 2013; Richardson 2013; Solomon 2001). For instance, because of this research, we now know that social values motivate what gets studied and what gets funded (Douglas 2009; Solomon 2001). We know social values motivate theory and method choice (Douglas 2009; Richardson 2013). For instance, a scientist who values career success can be motivated to choose their own method or theory for research. Social values guide what scientists pay attention to, emphasize, or deemphasize in their data (Bluhm 2013a; 2013b; Longino 2013). They are buried in the assumptions that traverse the gap between hypotheses and data, and so play a role in scientists' decisions about how well data support hypotheses (Longino 1990; 2002). Social values, recall from section 2.2, also influence the kind of scientific knowledge that people pay attention to. This can have a significant impact on theory acceptance as popular opinion can give the illusion of empirical success (Dupré 2001; 2012; Longino 2013; Meynell 2012). But all these lessons are lost when philosophers continue to take approaches to science like the truth-

detectional approach. The truth-detectional approach assumes a direct line between evidence and truth. Such an assumption prevents inquiry into the ways in which social processes and values disrupt this line or otherwise guide the production of scientific knowledge. The truth-detectional approach, therefore, narrows a philosopher's focus in their assessments and so compromises the quality and accuracy of their assessments.

By simply taking the truth-detectional approach, Buller, Richardson, and Gould risk compromising the quality of their assessments as described above. Buller, however, risks additional harms to the philosophy of science when he makes a pointed effort to delegitimize social-dimensional critiques of science. Consider Buller's treatment of those who have taken issue with the political dimensions of EP.

> ...as my research progressed, I became disheartened over the scarcity of reasoned intellectual exchange regarding evolutionary psychology. [...] it was too easy to find critics attacking evolutionary psychology for its 'directly political dimension' and its 'culturally pernicious' political claims. And, when evolutionary psychology wasn't being attacked on political grounds, it was easy to find critics dismissing evolutionary psychology for being built on a single 'fatal flaw.' [...] Thus dismissing evolutionary psychology for its corrupt politics or being based on 'one big mistake' enabled critics to deflect attention from the *evidence* that evolutionary psychologists present and to avoid altogether any serious engagement with evolutionary psychology. (Italics original 2005, 4)

As is clear from this excerpt, to Buller critiquing a scientist for the politically pernicious implications of their theory is not "reasoned intellectual exchange." This sends a clear message that addressing at least the political dimensions of a scientific theory is unsophisticated.

This message is harmful to the philosophy of science because it delegitimizes discourses that are in fact very valuable to the philosophy of science. Science *is* social and political, and addressing these dimensions can shed important light on the knowledge productions processes of science. Moreover, saying that discourses that address political values in science are unreasoned suggests that only those discourses that refrain from talk of values are reasonable. But never talking about values in science will inevitably result in peddling the very unreasonable notion that science is value free.

The message from Buller's excerpt is also unduly harmful to feminist philosophers who have criticized EP. Indeed, the one political analysis of EP that Buller footnotes in the quote above is a work that is co-authored by Hilary Rose (see the Introduction in Rose and Rose 2000), a feminist sociologist, and is very feminist in its content. Sarah Richardson (2010) discusses the myriad of ways in which feminist philosophers are a marginalized group within the philosophy of science. She explains that feminist perspectives of science are often simplified, caricatured, and thought to have little philosophical import beyond their diagnoses of "bias." Feminist critics of science are often villainized, she says, to have "anti-science" aims such as diminishing and limiting the influence of science, flat-out denying scientific findings (that don't uphold feminist values), and rejecting scientific values wholesale, such as objectivity, empirical verification, and logical reasoning (in favor of feminist ideology) (353). Buller's caricature of those who concern themselves with the political issues in EP contributes to this villainized image of feminist work.

Importantly, however, neither Buller's accusations nor those mentioned by Richardson (2010) are well grounded. Most feminist critiques of science aim to promote *better* science, not to get rid of it. It is true that feminists argue that there should be more feminist values in science, but these arguments are nuanced and take careful consideration of the ways in which science is a

social process and a social good. Contrary to how Buller characterizes feminist or political discussions about EP, feminists have provided their fair share of "serious" analysis. Over and above their extensive work on the specifically political dimensions of EP, feminists have also combed through EP studies to see how they measure up to the standards of evolutionary biology, have applied rigorous feminist analyses to the inappropriate assumptions evolutionary psychologists make, and have offered superior methods, theory, and data that evolutionary psychologists can and should make use of (see especially Dupré 2001; 2012; Eagly and Wood 1999; Fausto-Sterling 2000; Fausto-Sterling, Gowaty and Zuk 1997; Gannon 2002; Lloyd 2001; 2003; Lloyd and Feldman 2002; Meynell 2012; Rosser 1997; Sork 1997; also see discussions in Fehr 2011).

In sum, ignoring the social dimensions of science is harmful to the philosophy of science because doing so promotes an epistemological approach to science that is no longer tenable and compromises the quality of one's philosophical assessment. Effortfully delegitimizing social-dimensional approaches to science criticism in one's truth-detectional approach is also harmful to the philosophy of science because it acts to delegitimize an epistemological approach to science that is in fact quite valuable.

Conclusion

I've presented two philosophical approaches to the assessment of EP that stand as a case study for the assessment of morally relevant science more generally. The first approach, the truth-dectectional approach, is hyper-focused on the evidence provided by a science and how that evidence supports the truth claims of that science. The second approach, the social-dimensional approach, also considers the production and quality of a science's evidence but does so within a

framework of social processes and values. I pointed out that the truth-detectional approach is socially harmful since it ignores or cannot adequately address those dimensions of science that cause harm and so is unintentionally complicit with them. I also showed how the truth-detectional approach harms the philosophy of science because it assumes an epistemological approach to science that is no longer tenable and compromises the quality of one's philosophical assessment. The social-dimensional approach, in contrast, does not risk harms to society because it exposes and mitigates social harms embedded in science. Nor does it harm the philosophy of science because it engages a rich epistemological approach to science. I conclude, then, that the truth-detectional approach should be abandoned in favour of a social-dimensional approach for the assessment of morally relevant science. To avoid the pitfalls of the truth-detectional approach, philosophers of science ought to engage with and emulate the philosophical literature that sheds light on and emphasizes the importance of the various ways science is a social process and value-laden (for additional resources see, Brigandt 2015; Brown 2013; Hankinson Nelson and Nelson 1996; Kincaid et al. 2013; Longino 1990; Machamer and Wolters 2004; Solomon 2001).

I therefore commit to using the social-dimensional approach for my assessment of FEP. FEP is a morally relevant science that makes claims about women that could cause them harm. For instance, because FEP is a science that studies the "nature of women" (Fisher et al. 2013), many FEP scholars make claims about what women are like generally. But given that there are over three billion women in the world, living in a wide range of contexts, it is very difficult to make claims about women generally, and FEP scholars risk creating or perpetuating false stereotypes about women if their research is inaccurate. What is more, FEP scholars also risk causing harm to women who do not fit the "women-in-general" image FEP creates. These

women could be made to feel, or others could come to treat them as not "real" women. I revisit these concerns in a later chapter. In the next two chapters, Chapters 4 and 5, I apply use the social-dimensional approach to assess FEP theoretical foundations and data practices, respectively. I point out weaknesses regarding FEP's use of the BTP paradigm in Chapter 4, and show how FEP scholars too frequently offer the wrong data or not enough of the right kinds of data to support their adaptationist hypotheses. In both chapters I give recommendations for how FEP scholars can address the concerns I raise. In Chapter 6, I apply the social-dimensional approach to FEP to assess its values and social impact. I find evidence of values of Authority and Social-Order in FEP that are harmful in ways similar to the concerns I mention above. I offer recommendations for how FEP scholars can address my worries about their values.

Chapter 4
Critical Analysis of FEP Theory

4.1 Introduction

As the last chapter highlighted, evolutionary psychology (EP) has been criticized over the last decades by philosophers, scientists, and other scientifically concerned scholars for its substandard evolutionary science. Because feminist evolutionary psychologists still conduct their research broadly within the disciplinary framework of EP, their epistemic practices (e.g., theories, assumptions, data practices) may be flawed in similar ways that EP practices are. Over the next two chapters I assess FEP to see if this is the case and analyze the adequacy of FEP theory and data. I explore philosophical and scientific critique of EP theory and apply the themes from these critiques to FEP.

In this chapter, I focus on specific problems with the theoretical paradigm FEP scholars lean on to explain the behaviours they study, the Bateman-Trivers-Parker paradigm (the BTP paradigm) that I outlined in Chapter 2. Like other evolutionary psychologists, FEP scholars borrow heavily from this theoretical platform to initiate inquiry regarding and to explain sex differences in human behaviour. In recent decades, however, critics have pointed out serious problems with this paradigm. I explore three of the major criticisms from this literature in this chapter and discuss their relevance to FEP research.

To readdress the problems with the BTP paradigm that I outline throughout the chapter, I offer alternative theoretical platforms FEP scholars could explore. These platforms provide compelling evolutionary models for studying the kinds of human behaviours FEP scholars are interested in but are not limited in the same ways that the BTP paradigm is. For instance, unlike the BTP paradigm, which is restricted to explaining behaviour in terms of highly specific

adaptations, these models are designed to take into account a multiplicity of causal factors. This wider scope would help FEP scholars anticipate and make sense of, especially, human variation, including within-sex variation, variation across temporalities, environments, development, and individuals. The study of human behaviour is messy due to the ubiquity of variation. FEP research ought to be appropriately equipped for the task.

4.2 Problems with the BTP-paradigm

Recall from Chapter 2, the "Bateman Trivers Parker paradigm" (or BTP paradigm) that evolutionary psychologists often rely on to explain human sex differences. According to this paradigm, as laid I laid out on p. 23:

> (a) females are the higher investing sex because of the cost of their gametes, (b) males are generally more eager to mate and more indiscriminate than females, (c) male reproductive success is more variable than that of females, (d) males gain more in reproductive success from repeated matings than do females, (e) these differences in males and females will lead to sexual conflict (adaptations/counteradaptations). (a – d from Dewsbury 2005, 831)

In this section, I discuss criticisms that have been levelled against this paradigm and how some of these criticisms are relevant to FEP scholarship. As I will discuss more in the conclusion, there are ready alternatives to the BTP paradigm that are far more appropriate for FEP. These alternatives are not vulnerable to the criticisms I discuss in this chapter.

A first major issue with the BTP paradigm is that its supposedly universal generalizations about male and female mating behaviour are too often wrong. Many scholars have come forward

with evidence from species across the animal kingdom that contradict or complicate BTP descriptions of males and females. For instance, Ah-King and Gowaty (2015) report data from over 100 sexually reproducing species that suggest few to none of the species studied express the sex-typical, and presumably genetically determined, patterns of mating behaviour that the BTP paradigm describes. According to this data set, the sexes in the species examined appear to change their mating strategies depending on ecological or social demands. Thus, while males and females may sometimes appear to conform to the paradigm's assumed patterns, such conformity is superficial. Males and females may "swap" strategies or modify them in response to environmental demands.

Along similar lines, other scholars have offered reviews of wide varieties of species to see if male and female mating strategies corroborate, specifically, Trivers's parental investment theory. These scholars present much data on male and female behaviour that is contentious or inconclusive with regard to predictions based on Trivers's theory (Alonzo 2009; Kempenaers and Sheldon 1997; Sheldon 2002). For instance, Suzanne Alonzo (2009) reviewed 62 studies across 46 different species that addressed Trivers's prediction that males should decrease paternal care in response to paternity uncertainty. This prediction is based on the assumption that, because it is not in a male's interest to care for young, males should be especially careful to only care for those young that are in fact theirs, if and when they do care for young. Hence, when a male is uncertain that certain young are his, then he should invest less in those young. Alonzo, however, found that less than half of the 62 studies found support for this prediction whereas the majority found no significant effect of paternity uncertainty on paternal behaviour. Alonzo also reviewed 206 articles that tested the BTP prediction that females should choose mates with traits that indicate "the expected quality of male care" (101). Across these studies as well, only in 14 out of

the 27 species surveyed did females demonstrate a preference for males with traits that indicate paternal care. In the other 13 species, paternal care indicator traits were either negatively correlated with female choice or there was no relationship between the two. Data such as that presented by Alonzo and Gowaty and colleagues suggest the BTP paradigm should by no means play an *a priori* role in our reasoning about the natures of males and females (Alonzo 2009). The evidence suggests that BTP descriptions of male and female behaviours infrequently and only inconsistently obtain across and/or within species.

As a second criticism, some scholars have pointed out that strongly held BTP assumptions limit the scope of sexual strategies research. Critics in this domain argue that the BTP paradigm constrains the kinds of research questions that can be asked, and how observations can be interpreted. Regarding research questions, Ah-King and Gowaty (2015) note how leading up to the 1980s, most researchers of mating strategies simply wouldn't even explore questions that challenged BTP assumptions. Ah-King and Gowaty point out how loyalty to the BTP paradigm retarded for a long time consideration of, especially, "multiple mating in females, sources of variation in female reproductive success, the cost of sperm production, male mate choice and sexual selection in females at large" (219). Sarah Blaffer Hrdy (1988), in her critiques of BTP assumptions, argues something similar. She notes how BTP assumptions about females led researchers to overlook the different ways females competed with one another. The BTP paradigm assumes that it is predominantly males who compete over mating and resources. But loyalty to this assumption led researchers to neglect studying the many and important ways in which females also gain fitness benefits through within-sex competitions (e.g., competition over mates, number of mates, resources, social status).

Regarding interpretations of observations, Tang-Martinez and Rider (2005) suggest that an over-reliance on the BTP-paradigm has compromised how many researchers are able to "interpret reality with regards to male and female sexual behaviour" (822). They explain how unchecked acceptance of the BTP paradigm has led researchers to “ignore,” “disregard,” or “misinterpret” findings about male and female mating behaviours that are contrary to BTP expectations. Tang-Martinez and Rider report that some areas of research had difficulty being taken seriously by mainstream evolutionary biologists because findings within them contradicted BTP assumptions. These areas included studies about the costs of sperm production, the importance of male mate choice, and the ubiquity and importance of female activity in soliciting sexual encounters. On male mate choice, for instance, they report how because the BTP assumption that sperm production is unlimited was so widely accepted, “pioneering papers emphasizing the potential importance of male mate choice… were largely ignored” (824). They note a similar issue surrounding early studies that noted females to be sexually assertive, to actively seek multiple copulations, or to routinely mate with more than one male. These studies were often disregarded or misinterpreted. On this Tang-Martinez and Rider say,

> The stereotype of sexually restrained, highly discriminating females did not provide a theoretical framework that could make sense of these reports unless males were forcing unwilling females to mate. A common assumption was that EPCs [(extra-pair copulations)] were initiated by males intruding into neighboring territories and that females passively and reluctantly acquiesced to males’ sexual advances. EPCs…were regarded as male reproductive strategies that were probably detrimental to females. (825)

Sarah Blaffer Hrdy recounts her own difficulties interpreting observations of female solicitations. Early in her career Hrdy was studying langurs and noticed how some females would actively solicit males who were not "harem leaders." She says,

> At the time, I had no context for interpreting behaviour that merely seemed strange and incomprehensible to my Harvard-trained eyes. Only in time, did I come to realize that such wandering and such seemingly 'wanton' behavior were recurring events in the lives of langurs. (1988, 126)

A third problem with the BTP paradigm is that heavy reliance on it leads researchers to neglect the role that environment and sociality play in shaping behaviour. Beginning with Stephen Jay Gould and Richard Lewontin (1979), evolutionary scholars have taken issue with how the paradigm downplays the importance of individual flexibility and variation (see Buller 2005; Dupré 2001; Emlen and Oring 1977; Fausto-Sterling et al. 1997; Kitcher 1985; Lloyd 1999; 2003; Richardson 2007). Individual flexibility and variation are necessary for a species' survival and propagation in ever-changing social and environmental conditions. Thus selection must favor individuals who can adaptively respond to a variety of environmental challenges. The BTP paradigm is poorly suited to deal with individual flexibility and variation – particularly within-sex variation. Its focus on genetically determined, sex-specific adaptations, a focus, recall, that is referred to as "adaptationism" by BTP critics, provides only a highly generalized and rigid perspective of sexual strategies (Gould and Lewontin 1979). Questions about the ways in which organisms navigate and respond to their social and ecological environments are largely beyond the scope of the BTP paradigm. Critics in this area have noted that EP research, especially, is often void of any consideration of alternative non-adaptation explanations for

psychological/behavioural phenomena even when alternative explanations are quite obvious (Buller 2005; Dupré 2001; Gould 1997a; 1997b; Lloyd 1999; Richardson 2007).

In summary, three categories of critique of the BTP paradigm reveal (1) that its supposedly universal generalizations about male and female mating behaviour are often wrong, (2) and that depending on it can limit or distort the kinds of research questions one asks, how one interprets their observations, and (3) whether or how one considers other important causes of behaviour like environment and social factors.

4.3 Problematic use of the BTP-paradigm in FEP

It is surprising that, like in other areas of evolutionary psychology, many FEP scholars work within the BTP paradigm to study and explain human sex differences. This is surprising given the extent of criticism the paradigm has received. In a few ways, at least, the scholars in my FEP research sample who work from within the BTP paradigm, are sensitive to some of the criticisms of the BTP paradigm that I cited earlier. For instance, theoretical discussions by Buss and Schmitt (2011), Fisher (2013), and Moscovice (2013) acknowledge and emphasize the evolutionary importance of female intrasexual competition and strategic female promiscuity. Such discussion demonstrates that these FEP researchers (but also others) do not follow Bateman and Trivers in erroneously assuming that female reproductive success is relatively unaffected by number of mates or that sexual selection acts mainly on males. Nevertheless, many FEP scholars still rely on the BTP paradigm to an extent that compromises their research. Below I outline how some FEP research is vulnerable to the three criticisms I outlined earlier.

Regarding the first criticism, that the assumptions/descriptors regarding universal male and female behaviours based on the BTP paradigm are often wrong, some FEP scholars proceed

in their research as though said assumptions/descriptors are unquestionably true. One false assumption that many FEP scholars unwittingly take as fact, is the assumption that females have an evolved strategy to prefer males with lots of resources. Much FEP scholarship supports the hypothesis that because women are the higher investing sex, they should be choosey about whom they mate with and should specifically seek out partners who can provide resources for them and their offspring. Largely, this hypothesis translates into the expectation that women, by and large, prefer wealthier men. As was discussed earlier, however, evolutionary biologists have had a difficult time finding empirical support for the hypothesis that non-human females by and large prefer males who promise investment (Alonzo 2009; Ah-King and Gowaty 2015). Given this difficulty, one would assume that any researcher who wanted to extend this hypothesis to humans would have to do a fair amount of empirical ground work. Unfortunately, this is not the case in FEP. Eight of the 29 FEP articles in my research sample embrace the adequacy of this particular assumption without adequate empirical verification. For instance, Maryanne Fisher's (2013) hypothesis regarding women's mating strategies does just this. To describe men's and women's basic mating strategies, Fisher lays out the following:

> Women have faced sex-specific reproductive circumstances during evolutionary history; in contrast to men, women have energetically costly gametes that are comparatively few in number. Once fertilization occurs, it involves a substantial investment in terms of energy and time. Due to this differential in required reproductive effort, men's optimal reproductive strategy may be to seek as many matings as possible and invest little in any resulting children, while women's optimal strategy may be to carefully seek a mate and invest heavily in any children (as based on mammalian parental investment strategies as elaborated by Trivers, 1985). It is probable that these differences in reproductive strategy

> have influenced methods of competition. For example, men may compete for access to fertile women, while women may compete for access to men with resources and who are willing to provide paternal care (Buss, 1989; Cashdan, 1998). (24)

Fisher then adds to this account by discussing some EP literature that marks a distinction between male and female long- vs. short-term mating strategies. For women, she says,

> When seeking a brief interaction, women are thought to prefer a mate with high "gene quality" given that the man will presumably not invest any resources or parental care in any resulting children (see for example, Gangstad & Simpson, 2000). (24)

Based on these BTP-derived theories about short-term and long-term strategies, Fisher hypothesizes that women should seek wealthier, higher investing men in long-term contexts and men with "good genes" in short-term contexts. What Fisher doesn't do, however, is provide or discuss any evidence to support the possibility that women have *evolved* to have these preferences. She cites correlational data that suggests women have these preferences, but this is not the same as establishing a link between a preference and an evolved tendency. There are all kinds of reasons for human behavioural patterns—even patterns that appear to exist across cultures (as the Buss 1989 study that Fisher cites suggests). Simply finding robust patterns (granting for the moment that the patterns found in the research she cites are actually robust, but see Eagly and Wood 1989 for a critique) in human behaviour does not in itself prove that the pattern is the result of selection. More needs to be done in the way of establishing genetic linkage, or positive fitness outcomes associated with the behaviour (Lloyd 1999; 2002). Given the lack of empirical support for the BTP paradigm in relevant non-human evolutionary research and the lack of empirical support offered in the human research she cites, Fisher in unjustified in

using the BTP paradigm as a launching point for her investigations into women's mating-related behaviours.

In their studies on men's and women's flirting behaviours, Frisby et al. (2011) also make use of the BTP assumption that females prefer mates with resources to hypothesize that women "are attracted to men's resources and dominance, which are sought for personal and offspring protection" (685). They also hypothesize, based on the BTP assumption that, if and when males are choosey about mates, they will prefer physically attractive mates who appear fertile. As such they suggest "men are attracted to physical beauty and youth as characteristics that signal fertility and health, essential features for women to produce offspring." Connecting these BTP assumptions with Relational Framing Theory (RTF), which holds that all human social interactions are interpreted by individuals in terms of affiliation and dominance, Frisby et al. (2011) predict that men will prefer women who appear affiliative when they flirt and women will prefer men who appear dominant when they flirt. They reason that affiliation in a woman should signal caretaking qualities, and so be attractive to men, and dominance in a man should signal ability to protect and to accrue resources, and so should be attractive to women. They tested these hypotheses by having participants rate an individual of the opposite sex's photo before and after they watched a video of that same individual in a flirtatious encounter. Participants rated the individuals in the videos according to whether they thought the individual was physically attractive, socially attractive, dominant, affiliative, flirting for fun, flirting for sexual purposes, flirting to explore a relationship, flirting for self-esteem enhancing purposes, was conversationally effective.

They found that men's physical attraction for a woman in a photo increased if they perceived the woman to be affiliative when she flirted in the video. However, they found no

relationship between women's perceptions of men's physical attraction and how dominant they thought their flirting was. Thus, the expectation that women are attracted to dominant men because they can protect and provide resources is undermined. Curiously, however, when they later bring up another finding from their study that suggests the women participants are least attracted to men who they perceive to be flirting just for fun, they say the following:

> when men's flirtatious behavior was motivated by fun, women's perceptions of their physical attractiveness decreased in comparison to initial evaluations of the men's physical attractiveness. According to [parental investment theory], women are most attracted to potential mates who have resources they can, and will, invest in partner and offspring protection. Men who are perceived as simply flirting for fun may not be seen as sufficiently serious and available to invest these resources in a potential partner. (691)

I have two concerns with what Frisby et al. (2011) are doing in this excerpt. First, in no way are they justified in *explaining* the finding they mention in the excerpt by appealing to a BTP assumption very related to the assumption that was just undermined by an another finding of theirs. If, as they found, women's attraction to men does not track dominance, then perhaps women do not have an evolved disposition to partner with men who are resourceful and protective after all. Given this very real implication of their very own research, Frisby et al. should not be so quick to turn around and explain another finding by suggesting that it's because women have an evolved disposition to partner with men who are resourceful and protective.

Second, it is worth pointing out that what is missing from the discussion following the excerpt is an explanation for why some of the other findings regarding women's preferences also did not support the hypothesis that women should be attracted to resourceful and protective mates. For instance, Frisby et al. found that flirting for sexual purposes, flirting to explore a

relationship, or flirting for self-esteem enhancing purposes had no significant effect on the women's attractiveness ratings. But this is curious. Why is it that flirting for fun should make a man seem less likely to share his resources than a man who flirts just to have sex, or to enhance his self-esteem? There isn't any obvious promise of resources attached to these latter motivations either. Moreover, why weren't women *most* attracted to those men who flirted for the purpose of exploring a relationship? Surely, this motivation would be most connected to the possibility that a man would share his resources. Overall, this treatment of parental investment theory in Frisby et al. (2011) strongly suggests that the authors take much of its tenets to be simply true and generalizable to humans. Even in the face of their own evidence that undermines a certain aspect of parental investment theory, they are still quick to draw on that very aspect to explain women's behaviour in another but related context.

The widespread application of the assumption in FEP that women prefer men with resources is also problematic because it suggests an ignorance of the reproductive consequences that would come along with such a preference—i.e., a preference for *older* men. FEP scholars admit that men with resources are likely going to be older as the acquisition of resources takes time (Buss and Schmitt 2011; Low 2013). Despite this concession, FEP scholars do not discuss men's fertility in this respect or entertain the possibility that it might affect women's reproductive success. In contrast, FEP scholars talk at length about the significance of the limitations of women's fertility, limitations associated with, for instance, pregnancy, lactation, age, stage of reproductive cycle, weight, and activity levels (Buss and Schmitt 2011; Cameron and Garcia 2013; Escasa-Dorne et al. 2013; Johow et al. 2013; Low 2013; Meredith 2013; Pridmore-Brown 2013; Reiber 2013; Singh and Singh 2011). If men's fertility is ever discussed at all, it is usually mentioned as a means to emphasize women's higher investment. That is, because women's

fertility is so limited, they should be expected to be choosier with whom they mate than men who are assumed to *not* have such limitations. Men are even supposed to have adaptations for determining women's fertility. They are hypothesized to be attracted to women with "clear skin, smooth skin, facial adiposity, lustrous hair" because these features presumably indicate high fertility. Thus, fertility and being able to detect fertility (via youthful appearance) is generally said to be important in men's mate choices, but not for women's.[15] This seems to be based on the assumption that male fertility is not an issue so women do not need special adaptations to be able to detect it. What is more important, according to FEP scholars, is that women are able to detect traits in men that are conducive to acquiring resources.

The problem, of course, is that there are certainly limitations to male fertility, and there is no good reason to assume these limitations would not have evolutionary consequences. We know that men's fertility decreases quite sharply with age (e.g., Ford et al. 2000; Pennington 1988). Buller (2005) discusses this data and reports that among the !Kung of the Kalahari Desert and a large British sample, men's fertility peaks at about 30 and declines slightly until 40 but rapidly after that. Only 3% of fifty-year-old !Kung men were still able to have children, and men over 35 in the British sample were only half as likely to be able to impregnate their partners as were their 25 year-old-counterparts. I argue that this data has serious implications for FEP scholars' assumptions regarding women's evolutionary preferences for men with resources in particular. This consequence of women's alleged attraction to wealthier men is unproblematic if men's

[15] An exception to this is Frederick et al. (2013) who cite two articles that discuss the possibility that females may in fact have adaptations that facilitate an attraction to males with high fertility. However, these discussions are focused on non-human animals, and neither the articles nor male fertility are brought up when Frederick et al. discuss human female strategies.

fertility remained relatively stable into old age. But if it doesn't, as the !Kung and British data suggest, then it would in fact be evolutionarily disadvantageous for women to be attracted to older men. Focusing on choosing a man with resources to provide for you and your offspring is a futile strategy if that man cannot also provide you with offspring in the first place. In response, a FEP scholar might point out that it would still be evolutionarily advantageous for women to be attracted to older men with resources if they could also, on the side, utilize a short-term mating strategy to mate with men with "good genes." This way, women would get the best of both worlds, resources from their "cuckolded" older mate, and good quality offspring from their mate with good genes. The problem with this, however, is that the "good genes" hypothesis does not explicitly connect "good genes" traits to fertility or even to youth. Older men can also have features such as "bodily symmetry" or "masculine facial appearance," which are traits that supposedly signal "good genes" (Wilbur and Campbell 2013, 333). As such, this adaptation also does not do well to help women pick fertile mates. In sum, basing their discussions about human mating on the oversimplified assumptions about mating from the BTP-paradigm, leads FEP scholars to overlook important complexities about human reproduction. Contrary to FEP assumptions, male fertility does in fact have limitations and, like men, women might therefore need to know how to choose fertile mates. Relying on, without rigorously testing, the assumption that women are most concerned about finding a mate with resources, leads FEP scholars to miss this.

FEP research is also vulnerable to the second criticism of the BTP paradigm discussed earlier, that the paradigm constrains the kinds of research questions that can be asked, and how observations can be interpreted. Regarding the first of these, while FEP researchers have made substantial strides to direct evolutionary psychology research to focus on women and their

significance for evolution, just how they focus on women and which women they study remain narrowed by BTP assumptions. For example, because FEP researchers assume women are the higher investing sex they assume women are primarily caregivers, social supporters, and preoccupied with finding a long-term provisioning mate. As a result, FEP research reflects a hyper-focus on women in domestic contexts. In general, there is a dearth of discussions or studies of women in professional, political, technical, or competitive contexts (except for discussions of women's competitions over men). This is surprising especially in cases where FEP research is supposedly focused on women's aggression or competition (e.g., Fisher 2013; Liesen 2013). Even in these studies, discussions are limited to ways that women aggress or compete at home and over men. The lack of research questions about women's competitive performances and strategies outside of the home is alarming when we consider how often women compete, even aggressively, in non-domestic contexts (e.g., sports, at work, in academics). It is important to ask why women's competitive performances in these kinds of contexts are not relevant to FEP scholars. In non-domestic settings women are often competing over resources and social status. It is not obvious that these types of competitions are evolutionarily irrelevant. It appears, then, that BTP assumptions about sex roles—i.e., the competitive male and the discriminating, dependent female—are constraining how FEP scholars approach their topics of study. I will discuss the political issues regarding this in Chapter 6.

Regarding the criticism that the BTP paradigm constrains how observations can be interpreted, I have noticed some FEP scholars jump through hoops to try and fit their data to BTP expectations. For example, consider Wilbur and Campbell's attempts to explain, using parental investment theory, why men and women have different preferences regarding humour in a mate (reported in Wilbur and Campbell 2013 but based on studies from Wilbur and Campbell 2011).

Using survey data and data from dating websites, Wilbur and Campbell (2011) found evidence to suggest that women prefer a funny mate, but that men prefer a mate that finds them funny. To explain these findings, Wilbur and Campbell make use of the good genes hypothesis that is connected to parental investment theory. According to this hypothesis, the traits females find attractive are oftentimes "signals" to underlying genes that enhance fitness. Applying this to their finding that women prefer humorous men, Wilbur and Campbell reason that perhaps humour in a man is a signal to a woman that he possesses fitness enhancing traits such as cognitive flexibility, theory of mind, and social adroitness. To explain the finding that men prefer women who find them funny, they suggest, “Men’s reported preferences for women who appreciate their humorous attempts likely stem from humor appreciation signaling substantiation of men’s mate value” (2011, 10).

This latter interpretation regarding men’s preferences, however, is in fact quite contrary to a parental investment framework. There are three reasons for this. First, according to parental investment theory, because males are the lower investing sex, it pays for them to be less choosey and to focus on number of mates. If and when they are choosey, they will prefer mates that signal fertility and high offspring investment. Hence, if Wilbur and Campbell wish to connect men’s preferences for women who appreciate their humour to parental investment theory, they need to connect humour appreciation to fertility and/or offspring investment. But they make no attempt to do this. Because Wilbur and Campbell make no attempt to make this connection, their interpretation faces a second problem. It is unclear how men’s preferences for humour appreciation is any help to them fitness-wise. In fact, such a preference should be a *hindrance*. This is because men who are not found funny by women, according to Wilbur and Campbell's theory, are already at a disadvantage. If women are selecting men based on how funny they are,

then this means only a limited number of men—the funniest men—will be chosen. It does not make sense, then, to assume that the losers of this competition would be in any position to turn down the women who, despite not finding them funny, would be willing to mate with them anyway. Such a preference should all but destroy non-humorous men's chances at finding a mate.

Explaining men's preferences for humour appreciation in terms of parental investment and sexual selection flies in the face of another well-known aspect of sexual selection theory, the cheat. Cheats are individuals or species who have devised ways to obtain particular fitness outcomes without expending the amount of energy that is generally required for the outcome. For example, in cuttlefish, some males have figured out how to disguise themselves as females by changing their colouring to match typical female patterns. These males sneakily pass females their sperm sacs while other males are distracted, expending enormous amounts of energy, fighting over mating opportunities (Norman et al. 1999). With this in mind, if humour really is a type of mating effort, and men need to compete over women by using humorous tactics, then it should be in a man's interest to not use humour if he can get away with it. Having a preference for only women who find them funny, however, would preclude this. Thus, such a preference is irrational from a sexual selection vantage point for yet another reason.

Shoehorning their data to fit the BTP paradigm compromises Wilbur and Campbell's ability to adequately make sense of their data. On close analysis of Wilbur and Campbell's findings, it is apparent that the finding that men prefer women who find them funny is nonsensical within a BTP framework. Unfortunately, however, Wilbur and Campbell don't seem aware of this, perhaps because they are too committed to the BTP paradigm. As a result, they

settle for a highly dubious explanation, missing an opportunity to explore other more likely explanations.

Regarding the third criticism, FEP research often neglects to engage social, developmental, or ecological causes of behaviour. This shortcoming appears also to be a result of FEP reliance on the BTP paradigm as the paradigm is largely concerned about the ways in which sex-specific adaptations shape behaviour. The following examples illustrate some problems that arise when FEP scholars neglect to consider certain likely causes of behaviour because they are overly focused on possible adaptive causes. As I'll point out, FEP neglect of social, developmental, and ecological causes of behaviour leads to an oversimplified or even mistaken understanding of the behaviour in question.

Some FEP scholars make some effort to consider causes other than adaptation in their studies of human behaviour. Unfortunately, however, sometimes this effort amounts to little more than lip-service to these other possible causes. Elizabeth Lloyd (1999) criticizes this tendency among other evolutionary psychologists. Because evolutionary psychologists have been so frequently criticized for completely ignoring other factors that might influence behaviour, they have responded by building in minimal discussions about other factors like culture or environment. However, as Lloyd (1999) points out, these discussions are often so minimal that they do not really count as genuine attempts to explore or eliminate competing hypotheses. It is as though evolutionary psychologists only include them to say that they did. An example of this from FEP scholarship is Karolina Sylwester and Bogusław Pawłowski's (2011) brief treatment of social factors in their evolutionary account of risk taking.

Sylwester and Pawlowski (2011) report survey data which indicates that men and women are similarly attracted to risk takers in short-term mating contexts, and provide an adaptationist

explanation for this finding. Specifically, they posit different evolutionary motivations across the genders for the similar preferences. To explain women's preferences, they apply theory from Belsky et al. (1991) that suggests women, in short-term mating contexts, will prefer men with "good genes" (i.e., genes that will enhance the fitness or survival of their offspring). Sylwester and Pawlowski point to some evidence to suggest that certain forms of risk taking in men, such as taking physical risks that require strength, are indicative of good genes. Thus, they say, from a sexual selection vantage point, women should be attracted to risk-taking men in short-term (non-committal) contexts. They reason, what the females' offspring may lose in investment in a short-term context, they will make up for in good genes. Their reasoning for men's mating preferences for female risk takers, however, differs quite drastically. They suggest that men prefer risk-taking women in short-term contexts because risk taking signals sexual access. For this they provide evidence that suggests women who take risks such as binge-drinking are more likely to engage in casual sex. According to sexual selection, then, they suggest that it pays for men to be attracted to women who take risks as it reduces men's "mating effort" (703).[16]

At the beginning of their study, Sylwester and Pawlowski briefly introduce an alternative account for men's and women's mating preferences, one that highlights the effects that social structures (i.e., gender-specific "social roles") have on mate preferences. They, however, quickly dismiss this theory on account that it does not offer a theoretical picture for preferences in *short-*

[16] An unstated assumption here is that women's adaptive mating strategies are not similarly concerned about mating effort since there are always eager males willing to mate with them. There are more problems with how this study tries to apply to the good genes hypothesis to sex differences in risk taking. I talk more about these problems in Chapter 6.

term contexts. They also report some evidence which suggests risk taking behaviour to be more under the control of "biological determinants" rather than social ones (670).[17]

This brief treatment of "social role theory" (as the theory is called) as a means to address environmental/learning factors as an alternative explanation for their data is unsatisfactory. In particular, the content of social role theory does not adequately encompass the more obvious social factors that seem to be at play in Sylwester and Pawlowski's data. The immediate alternative non-evolutionary explanation for their results seems to have more to do with general social expectations about romantic short-term encounters and relationships. The stimuli in Sylwester and Pawlowski's study defined a short-term partner as "one with whom a participant would have casual sex or an affair" (700). Given that "casual sex" and "affairs" are already culturally loaded as exciting or risky types of relationships, it makes sense that people would imagine being in such a scenario with someone who would likewise be willing to be in an exciting, risky relationship. This, by definition, would make their imagined partner a risk taker. It is surprising that Sylwester and Pawlowski do not explore alternative accounts like the one I've given here that consider obvious non-adaptive causes for their findings. And bringing up social role theory as a way to deal with objections regarding social causes is a red herring. If the authors really wanted to get to the bottom of why people prefer to have casual sex or affairs with risk takers, they would allot more time to contending with the most relevant and likely competing explanations.

There are FEP scholars who, yet, do even less than Sylwester and Pawlowski in terms of taking into consideration causes of behaviour other than sexual selection. Remember Wilbur and

[17] This is actually quite confusing, though, since being biologically inclined to risky behavior is not the same thing as wanting to mate with someone who is a risk taker.

Campbell's (2013) study regarding mate preferences for humour. They hypothesize that women's choice for humorous men explains why women, but not men, prefer humour in a mate, and that men, but not women, prefer mates who find them humorous. Interestingly, at no point in their study do they make any mention of the potential role for social or environmental factors in shaping men's and women's valuations of humour in a partner. I suggest that such oversight is highly inappropriate given the obvious relevancy of social factors in their topic of study, in particular, the relevance of socially enforced gender roles. Some of the psychological qualities that humour signals, such as creative intelligence and mischievousness, are qualities that for a long time have been socially condoned in men but discouraged in women. A patriarchal social structure would have it that men would feel threatened by women who express creative intelligence and mischievousness. Such behaviours in women would make them seem unpredictable and capable of challenging rules that keep them in subordinate positions. Women, on the other hand, according to this account, should be free to find humor in a partner attractive. Because women do not have the same pressure to be *more* intelligent and creative than men, they need not find these qualities threatening, and can even enjoy them in a partner. Wilbur and Campbell lose out when they focus narrowly on sexual selection theory and fail to entertain other relevant and obvious explanations like the one I offer. Other FEP scholars who neglect entirely to entertain competing explanations for conclusions about human behaviour include Betzig (2013), Coe and Palmer (2013), Liesen (2013), Oberzaucher (2013).

The BTP paradigm's focus on genetics and adaptations markedly limits its scope, especially in regards to human behaviour. Human behaviours are complex and shaped by a multiplicity of factors. Especially behaviours like mating behaviour should be expected to be deeply influenced by social norms and individual circumstances. Psychology is burgeoning with

data which suggest that human sexual preferences are highly influenced by perceptions about social norms, and especially gender norms (e.g., Finkel and Eastwick 2009; Hundhammer and Musswiler 2012; Pedersen, Putcha-Bhagavatula, Miller 2011; Perrin et al. 2011; see also Brown, Laland, Mulder 2009 for a meta-review of studies that indicate a lack of universality in mate preferences across cultures). It is surprising that feminist evolutionary psychologists do not acknowledge or make use of such insights, especially when they come from within their own discipline.

4.4 Recommendations

For a long time, a focus on adaptations due to sexual selection—as sexual selection is characterized in the BTP paradigm—was standard for studying sex and sex differences in evolutionary biology. As I have discussed in this chapter, however, there are problems with this standard approach. As critics have pointed out, the paradigm has limited usefulness for predicting or explaining animal mating behaviour since so many species have complex and highly flexible behavioural repertoires. The BTP paradigm, because it frames mating behaviour almost exclusively in terms of universal, highly specified traits, is not equipped to deal with the kinds of complexity and flexibility related to reproduction and mating that has been observed in so many species.

FEP scholars present themselves as experts on the evolution of human behaviour and so should be cautious to engage theoretical platforms that are highly problematic like the BTP paradigm. Throughout this chapter I discussed how FEP's use of the BTP paradigm has resulted in problems that mirror those talked about by critics of EP. First, I pointed out how FEP applications of the BTP paradigm to women's mating strategies has led FEP scholars to overlook

important aspects of human reproduction. FEP expectations that women have evolved to prefer (older) men with resources, or men with "good genes," are not flexible enough to account for the fact that women also need to be able to detect fertile men, for instance. Second, I talked about the ways in which FEP adherence to the BTP paradigm leads to confusing and illogical interpretations of data. Finally, third, I showed that when FEP scholars avoid or are ignorant of non-adaptive causal mechanisms of behaviour, they do science a disfavour since they miss out on offering more complete explanations of the phenomena they study.

By taking on the role of expert, FEP scholars have an obligation to be up to date on the theoretical accounts of the phenomena they are interested in. In this vein, I would recommend FEP scholars familiarize themselves with the critiques of the BTP paradigm that I have cited and built on in this chapter. As well, I suggest FEP would do well to adopt as part of its program alternative theories for studying sex from an evolutionary perspective that are not vulnerable to the criticisms above. I provide three of these below. These platforms provide compelling models of flexible sexual strategies, so are not limited by the assumption that sexual strategies are fixed across the sexes (pertaining to criticism 1). They also have considerations of multiple types of causes of behaviour (besides natural and sexual selection) built right into them. Thus, while the models are evolutionary models they do not exclude consideration of canonically non-evolutionary causes (e.g., learning, environmental) as the BTP paradigm does (pertaining to criticism 3). Adopting these models would give FEP scholars a broader range of causal phenomena to consider in their studies of the evolution of behaviour. Moreover, FEP theory would be better suited to anticipate and explain human variation in their studies including within-sex variation, variation across temporalities, environments, development, and individual differences more generally (pertaining to criticism 2).

1. *Sex as a Reaction Norm.* Work by Sutherland, Johnson, Patricia Gowaty, Hubbell, and Malin Ah-King suggest that because demographic environments vary stochastically, selection should inevitably select against fixed sexual strategies and favour instead flexible individuals who are able to adaptively modify their strategies as their circumstances change (Ah-King and Gowaty 2015; Gowaty 2013; Gowaty and Hubbell 2005; 2009; Hubbell and Johnson 1987; Sutherland 1985). Hence, they propose the "adaptive flexibility" of *individuals* which is the idea that "selection acted so that all individuals sense and respond to the changing environmental and social circumstances that affect their options for reproduction and survival" (Gowaty 2013, 93). Assuming this set of initial conditions is to largely reject the idea that *males*, as a category, evolved in response to male-specific environmental challenges, and *females*, as another category, evolved in response to female-specific environmental challenges. Thus, when one observes a female being "choosey," one should not assume she is being choosey because the size of her gametes make it so, but because she has sensed from her environment that such behaviour is beneficial. If her environment were to change, she might very well choose to be "promiscuous." Gowaty and colleagues have tested this model extensively using computer simulations. Their tests show that random factors (e.g., encounters with potential mates, predation risk, disease risk, access to resources) can sufficiently account for differences in mating success variance between the sexes, as opposed to fixed sex-specific strategies. Ah-King and Gowaty (2015) also give empirical evidence for the model. I mentioned earlier in the chapter that in their review of over 100 non-human animal studies conducted between 2000 and 2015, they found that individuals across many species often switch from choosey to random mating when their social or

environmental circumstances change. This undermines the idea that one sex is inherently overall more choosey than the other.[18]

Worth noting, Gowaty (2013) is a chapter in *Evolution's Empress*, the anthology from which much of the FEP research in my literature sample comes from. Gowaty's discussion of her work on sex as a reaction norm makes her chapter stand out in the anthology as it is one of just two chapters in the book that offers extensive meta-theoretical critique of the BTP paradigm.

2. *Social Selection*. Quoting Darwin, Joan Roughgarden points out that the "chief service rendered" by reproductive social behaviour is to fashion a social infrastructure that produces an increased number of offspring (2015, 95). Following this, she develops what she calls "social selection" which is intended to be an alternative to sexual selection that better takes into account the role sociality plays in animal reproduction and evolution. Whereas sexual selection begins with gene pool dynamics (evolved sex differences e.g., coy, indiscriminate) to explain behaviour, social selection begins with behaviour to explain gene pool dynamics. Two starting considerations are taken into account. First, the ecological conditions into which the offspring are released, and the types of survival/fitness enhancing actions within this environment that the offspring are able to express, are considered. Second, the types of ornaments, signals and negotiation that are needed to bring about those actions during courtship are considered. Once these fitness solutions are anticipated, one can then "play the predictions forward and observe the optimal courtship followed by the optimal parental care" (Roughgarden 2015, 96). To Roughgarden, thinking about sex roles in terms of social selection helps highlight the importance

[18] Disappointingly, however, Gowaty 2013 notes that the authors of many of these studies often "erect ad hoc, usually correlational hypotheses to tack onto [Bateman-Trivers] hypotheses to explain observations" (88).

of negotiation. She says, because parenting roles need to be negotiated, sex roles cannot be explained in terms of fixed sex-specific traits. Thus, she says,

> Rather than thinking of the phenotype as a static product of the genotype, in social selection the phenotype is assumed to have its own dynamic, not only the solitary dynamic realized during development and morphogenesis, but also a social dynamic carried out within the lifespans of the interacting individuals. This interactive phenotypic dynamic shapes the properties and capabilities of the animals just as surely as morphogenesis shapes anatomy. (2015, 98)

3. *The Extended Synthesis.* Leslie Heywood, an English Professor at Binghampton University, is interested in developing research perspectives that integrate theoretical insights from humanities topics such as representation and social construction with evolutionary approaches. Perhaps unsurprisingly, she has affiliations with feminist evolutionary psychologists and even has a chapter in *Evolution's Empress*. Her chapter in *Evolution's Empress* stands out for a similar reason that Gowaty's (mentioned above) does. Like Gowaty's, her chapter offers a critical, meta-theoretic discussion of BTP evolutionary theory and offers alternative ways to understand and study sex and gender from an evolutionary perspective. She provides what she calls the "extended synthesis" for human evolutionary study. This proposed theoretical paradigm incorporates insights from neuroscience, genetics, developmental psychology, and paleoecology. It is built on the assumption that "biology and culture are inextricable" (Heywood 2013, 456). Heywood proposes niche construction and evolutionary developmental biology as important theoretical sources for the evolutionary study of sex roles. Niche construction is concerned with the ways in which organisms engineer their environments to enhance their survival. An organism can engineer its environment within its own life such that the changes made affect the

development of and how selection acts on its descendants. Humans rely on a variety of inheritance systems for the passing down of niche construction including genetic, epigenetic, behavioural, and symbolic systems. Heywood explains that niche construction has implications for some of the novel sex, gender, and reproductive patterns emerging in humans. She says,

> technological advancement has so altered the human environment that resource acquisition and provisioning have changed profoundly, and 'fast' evolution shows that there has in fact been time to alter patterns such as those claimed by [parental investment theory]. (Jablonka and Lamb 2005, as cited in Heywood 203, 456)

Adding to this, Heywood points to research from evolutionary developmental biology, which draws on recent trends in developmental biology and cognitive neuroscience, that shows how extensively individual and social learning shape the brain and behaviour. Taken together, insights from niche construction and evolutionary developmental biology have major implications for sexual selection research. They undermine attempts to explain even widespread patterns of sex differences by appeals to sexual selection. If key causal processes of sexual behaviour primarily occur at the developmental and social levels, then pondering the specific conditions of ancestral times is unhelpful as these conditions would have led to very different behavioural outcomes than the ones emerging today.

FEP research ought to explore the suitability of new developing theoretical frameworks, such as the ones I have just provided. Adopting any one of these models would benefit FEP research. Each have fewer preconceived expectations about sexual strategies because they anticipate that organisms will develop their strategies based on a variety of inputs including genetic, epigenetic, developmental, and social. Employing these models would also give FEP research more breathing room to explore aspects of sex research that are not accessible through a

sexual selection lens, such as variation within the sexes and across individuals, social and environmental factors (that need not loop back to sexual selection somehow), and development.

Chapter 5
Critical Analysis of FEP Data

5.1 Introduction

In the last chapter, I looked at problems associated with the Bateman-Trivers-Parker (BTP) paradigm that is the predominant theoretical paradigm FEP scholars employ in their research. I brought attention to the criticisms of the paradigm and showed how these criticisms apply to FEP research. In this chapter, I focus my analysis on the appropriateness of the data feminist evolutionary psychologists offer for their adaptationist hypotheses. In the first part of the chapter, I point out ways FEP scholars offer the wrong data. In the second part, I point out how FEP scholars do not offer enough of the right kinds of data. To make my assessments, I draw on past philosophical criticisms of the appropriateness of EP data, but also develop an additional category of critique. This new category concerns the success of feminist evolutionary psychologists' attempts to operationalize evolutionary terms in the context of human study. I call this focus of critique the OEP check (OEP stands for operationalization of evolutionary phenomena). While my criticisms in this regard are specific to FEP in this chapter, the OEP check is an important tool for anyone who conducts or assesses human evolutionary research. I develop and apply this critique in the first section of this chapter. In the remaining sections, I show how past and well-known criticisms of EP data are relevant to FEP. This is concerning, I point out, as the mistakes regarding data the philosophers criticize are basic mistakes that should not be happening in scientific research at the rate that they do in EP. Feminist evolutionary psychologists would do well to pay attention to these criticisms. In each section of this chapter, I offer recommendations for how FEP scholars might address the weaknesses regarding their data that I critique.

5.2 The Wrong Data

5.2.1 Problems Operationalizing Evolutionary Phenomena

In this section, I develop a novel critical concern regarding appropriateness of data in human evolutionary research. I address the problems FEP scholars face when they try to operationalize evolutionary phenomena with observable human behaviours. I argue FEP scholars should be more rigorous when they operationalize the evolutionary terms they borrow from evolutionary biology as they are frequently unsuccessful at demonstrating an adequate link between the evolutionary phenomenon they are attempting to measure and the proxy phenomenon they are actually assessing. As a result, they end up collecting data that is not relevant to the evolutionary phenomenon they are studying. While I apply this critique to FEP scholarship only in this chapter, it is a concern that should be relevant to any research on human evolution, especially human evolutionary research that borrows terms from non-human animal research. More on this later.

Operationalization is the process of defining the measurement of a phenomenon that is not directly measurable, though its existence is indicated by other phenomena. In other words, it is the attempt to measure a phenomenon that is highly complex or elusive by measuring a more accessible phenomenon that is assumed to be related to it. An example would be the medical industry's attempts to measure the very elusive and complex phenomenon 'health' with BMI, an easy-to-measure feature of people that some think is indicative of overall health. There are many phenomena in evolutionary biology that are elusive or very complex (e.g., evolution itself, mate quality, parental investment, adaptation). Those who study evolutionary biology are tasked with the job of quantifying and measuring these hard-to-measure phenomena by finding and

measuring their more accessible proxy phenomena. For instance, evolutionary biologists who measure male ‘parental investment’ in reed buntings will assess the rate at which a male will feed his young. A male who is less invested in his young would probably spend less time foraging for his young, and more time for himself, than a male who is highly invested. Investment in feeding has direct fitness consequences and it is one of only a few ways a male reed bunting can invest. Thus, how much food a male reed bunting feeds his offspring is a good indication of his parental investment (Dixon et al. 1994, but also see Dickinson 2003 for critique of generalizing this tendency to other species).

Evolutionary psychologists must do the same with humans. And because they borrow theories and terminology directly from evolutionary biology, evolutionary psychologists must find human equivalents for the proxy phenomena evolutionary biologists study. This, however, is not easy. Evolutionary psychologists face a number of disadvantages relative to researchers who study non-human animals. Factors such as the human lifespan, limits to and ethical limits on human observation, and the type of culture humans have make the evolutionary study of humans quite difficult and unique from much non-human study. Returning to ‘parental investment,’ for example, this concept is far more difficult to operationalize in humans. There are countless ways human fathers can invest in their children and not all of these ways are obviously fitness enhancing (this would in fact be quite hard to determine). Assessing men on one or even five aspects of "investment" would be insufficient since a father who demonstrates low investment in these aspects could demonstrate high investment in unknown or unobserved others. What is more, how or why a father invests in his children is very culturally and historically determined. A picture of "paternal investment" in Canadian society today may look very different from how it looks in other cultures or would have looked 100 years ago. Such a study would at the very

least have to compare fathering investment behaviours across multiple domains of investment, and across cultures and time periods to be representative.

There is evidence in FEP that FEP scholars need to be more rigorous when they attempt to operationalize evolutionary phenomena with measurable human behaviours. Too frequently they fail to develop an adequate link between the evolutionary phenomenon they are attempting to measure and the proxy phenomenon they are proposing is indicative of it. Below are three examples of FEP scholars falling short in this way.

Example 1: "mating power" as "social power"

Recall from my discussions in Chapter 1 Julie Seaman's (2013) application of sexual selection theory and female choice to human gendered power dynamics surrounding dress codes. In her paper, Seaman operationalizes an evolutionary sense of "power," however I argue she has difficulty establishing its link with observable human behaviour.

Seaman points to a phenomenon in nature where the most elaborately ornamented sex (e.g., the most colorful plumage) is often the sex with the least amount of "power," where power specifically refers to choice in mating. This observation comes out of the BTP paradigm which posits that elaborate dress in males is usually the result of female choice. That is, because females in many species, but not the males, can freely choose their mates,, the "dress" of the male will become honed to cater to her preferences. Accordingly, the female in this scenario holds the power since she has the control in the mating arena.

Seaman the goes on to suggest that this feature of sexual selection theory can inform how we understand and deal with the human gendered power dynamics that result from dress codes in the workplace. She notes that Because men have the power to demand it of them, women are usually the more elaborately dressed. As a result, women's choices are constrained, and this has

consequences for their reproduction and fitness. She concludes by explaining how, when we view cultural dress through this evolutionary lens, it becomes clear that enforced dress codes in the workplace reinforces gender stereotypes, violates women's autonomy, and leads to sex discrimination.

While I certainly do not disagree with Seaman that gendered dress norms are harmful, I am concerned that the path by which she arrives at this conclusion is unhelpful because it merely conflates two very different notions of "power." In the non-human context, the evolutionary sense of power refers to *choice of mates*. But in the human context, Seaman indicates the word takes on a host of other, very different references—none of which have to do with actually choosing a mate. This is made more confusing when we consider the canonical view in evolutionary psychology which states that it is women who supposedly have the most mating power since they are choosey and men are less discriminate. Seaman cites some of the EP research that puts forward this view, so she is aware that mating power has been applied to humans in this sense. Seaman's operationalizing "power" by using the proxy "human dress" does not track what she defines as evolutionary "power" since the former has nothing to do with what the latter indicates, choice of mates. As a result, I argue that Seaman is unsuccessful in establishing an ontological relationship between "power" in the evolutionary sense and "power" in the political sense because the two are just the same word but for very different phenomena.

Example 2: parenting strategies

Nicole Cameron and Justin Garcia (2013) apply the evolutionary terms "qualitative" and "quantitative" parenting strategies in the context of humans. Theory from non-human animal studies on these strategies suggests that parents who experience favorable environmental conditions will sacrifice an emphasis on mating and opt for "qualitative" parenting which yields

a high investment for each individual offspring (also called the "K strategy"). Parents who experience adverse environmental conditions where risk for mortality is high, on the other hand, will switch their focus to mating and "quantitative" parenting in which the goal is to maximize number of offspring to increase the chances that at least a few will survive (a strategy referred to as the "r strategy").

Cameron and Garcia find evidence for their application of this theory to humans in the data which suggests "environmental adversity is associated with a decreased quality of parental care," such that, "[i]nsecure attachment, child neglect, and child abuse are more prevalent in populations living under impoverished conditions" (e.g., Belsky 2010 as cited in Cameron and Garcia 2013, 141). They also cite evidence which suggests that females who grow up in "an impoverished family environment" experience early menarche and earlier onset of sexual activity" (e.g., Griffin and Harlow 1966 as cited in Cameron and Garcia 2013, 141).

I would, however, like to draw attention to the important difference between having lots of children (or having a propensity to focus on mating) and raising your children badly. What is not clear in Cameron and Garcia's presentation of both the human and non-human animal literature on K and r strategies is that quantitative parenting is equivalent to *non-qualitative* parenting. Employing a strategy to maximize one's offspring does not *entail* that one will mistreat them once they are born. There might be factors associated with an "adverse environment" such as stress, ill health, mental illness, and substance abuse that contribute to the incidence of child abuse, but these factors are not equivalent to a quantitative mating strategy. All a quantitative strategy entails, proximately speaking, is a preoccupation with mating. Thus, Cameron and Garcia fail to appropriately operationalize quantitative mating when they choose to focus on bad parenting as a proxy.

Example 3: "good genes" as "humour"

Wilbur and Campbell (2013) engage the sexual selection literature that emphasizes the significance of female choice. This literature suggests that female preferences in many species track good genes in potential mates, where the features females find most attractive happen to be features that signal fitness enhancing traits. Recall, Wilbur and Campbell are interested in women's preferences for humorous men since humour is hypothesized to signal skills that indicate mate quality: cognitive flexibility, theory of mind, and social adroitness. Based on their findings, they suggest that, as is the case in other species like the peacock, women's mate preferences, through sexual selection, can shape the physiologies and psychologies of their mates. Wilbur and Campbell conclude that women's preferences for humorous men have led to a general tendency among men, but not women, to strive to be funny when trying to attract a mate.

Focusing on the operationalization of "mate quality," Wilbur and Campbell have chosen to measure this known evolutionary phenomenon with "humor," so let's look at their justification for that link. "Mate quality" refers to those features of an organism that could affect the reproductive success of its mates. In order to link "humor" to "mate quality," Wilbur and Campbell go through the following steps (see pages 337-340): (1) they cite G. F. Miller (2007) who conjectures that humour evolved as a fitness indicator because it requires adaptive skills such as cognitive flexibility, theory of mind, and social adroitness. (2) They suggest that, to females (but not to males), humor is an honest signal because it "tacitly" conveys qualities such as "intelligence" and "warmth" which are "highly valued traits in romantic partners" (338). The emphasis on "tacit" is important because humor is supposed to *demonstrate* intelligence and warmth which is much more reliable than testimony (hence why it is an honest signal). (3)

Wilbur and Campbell found a link between "intelligence" and "humor" because men on a dating website who advertised that they were intelligent also tended to advertise that they were funny.

One major problem with Wilbur and Campbell's link between mate quality and humor is that they do not give us an account for how the cognitive traits associated with humor (cognitive flexibility, social adroitness, theory of mind) can lead to reproductive benefits for women *through* their association with humor. It is plausible to imagine how cognitive flexibility, social adroitness, and theory of mind could have fitness benefits, but it is not clear that their affiliation with humor is a good vehicle for their being passed along. For example, the kind of cognitive flexibility needed to make a play on words might not be the kind of cognitive flexibility that someone needs to raise a child or build a shelter. Likewise, while social adroitness seems necessary for cooperative living, it isn't clear that the adroitness associated with entertaining people is inherently cooperative. In fact, some research indicates that comedians have a high incidence of depression and anxiety—two disorders that complicate social relationships (e.g., Janus 1975; McBride 2004). Comedians also score low on extroversion and agreeableness which, according to Greegross and Miller (2009), indicates that comedians prefer to keep to themselves and are distrustful of others. Regarding theory of mind, again, it is not clear why humor is an especially good indicator of this. Other non-human animal species possess theory of mind (see Keefner 2016), but they don't need humor to facilitate its selection. Given the presence of theory of mind in other species, I suggest it seems more likely that the proxy traits that are responsible for the fixation of theory of mind in humans came long before humour. In sum, because Wilbur and Campbell do not develop an adequate link between humor and its alleged fitness enhancing traits, their attempt to operationalize mate quality with humor is unsuccessful.

In sum, FEP scholars are frequently unsuccessful at demonstrating an adequate link between the evolutionary phenomenon they are attempting to measure and the proxy phenomenon they are actually assessing. As a result, it is unclear whether they are collecting the right data to support their evolutionary hypotheses. I suggest FEP scholars be more vigilant about this aspect of their research. I recommend they dedicate more space in their papers to explaining and establishing the link that they perceive exists between the evolutionary phenomenon they wish to assess and the accessible human phenomenon they are measuring. Spending more time thinking about this link would save FEP scholars from pursuing data that is irrelevant to their hypothesis.

I also recommend FEP scholars be more cautious when they operationalize terms that they borrow from non-human animal research. As I mentioned previously, evolutionary psychologists face challenges studying evolution in humans that other evolutionary biologists do not. As a result, they should be extra careful when they are defining the human equivalents of the proxy phenomena evolutionary biologists study. For instance, as we saw in Seaman's (2013) and Cameron and Garcia's (2013) cases, the kinds of behaviours non-human animals display that are indicative of "mating power" and "K and r strategies," respectively, are not clearly mirrored in humans. Zoomorphizing is when researchers wrongfully attribute characteristics that are specific to certain non-human animal species to humans. FEP scholars need to take this type of problem seriously. Because the terms and concepts in much evolutionary research have been developed in the context of non-human animal studies, they might not be appropriate for the study of humans. If researchers of human evolution cannot secure a research environment and methodology that can yield as effective a measure of the evolutionary concept they mean to study, then they should consider abandoning the concept and adopting or inventing one that is more conducive to human

study. This avoids confusion and the unwarranted epistemic credibility that sometimes comes along with borrowing familiar successful terminology that might not be obviously context dependent.

In the next section, I discuss a second issue in FEP that regards FEP scholars collecting the wrong kinds of data for adaptationist hypotheses. For this critique, I draw on past philosophical criticisms of EP.

5.2.2 Just-So Stories

Stephen Jay Gould and Richard Lewontin (1979) took issue with a kind of data evolutionary psychologists sometimes offer, a kind Gould and Lewontin call "just-so stories." A just-so story is an attempt to establish an evolutionary function of a trait by telling a plausible story about what problem the trait needed to solve in an ancestral past. The problem with this, according to philosophers, is that a just-so story is not the right kind of data needed to support a claim regarding evolution by natural selection. For instance, Gould (1997b) criticizes Robert Wright, an evolutionary psychologist who offers a just-so story to support an adaptive claim about the "sweet tooth." Wright (1994) explains, "The classic example of an adaptation that has outlived its logic is the sweet tooth. Our fondness for sweetness was designed for an environment in which fruit existed but candy didn't" (as cited in Gould 1997b, n.p.). Gould responds to Wright's reasoning by pointing out how it,

> ranks as pure guesswork in the cocktail party mode; Wright presents no neurological evidence of a brain module for sweetness, and no paleontological data about ancestral feeding. This 'just-so story' therefore cannot stand as a 'classic example of an adaptation' in any sense deserving the name of science. (1997b, n.p.)

A number of the FEP scholars in my literature sample use just-so stories as data to support their evolutionary claims, and I can criticize their attempts for similar reasons Gould does. Consider (1) Coe and Palmer's (2013) just-so story about the function of ancestral names, and (2) Pridmore-Brown's (2013) just-so story about the evolution of patriarchy:

1. We suggest that a key benefit to traditions sometime between 100,000 years ago and 40,000 years ago was the identification of kin, through such things as ancestral names (e.g., clan and tribal names) and other identifiers of common ancestry, such as tribal dress and or other body decoration. This identification, when combined with traditional behaviour that encourages cooperation among those identified as kin, had the crucial advantage of producing cooperation between second and perhaps even third cousins, and thus larger cooperating sets of allies. (118)
2. ...while patriarchy may well be socially constructed, its condition of possibility is the gendered reproductive clock, which has been instrumental in stabilizing patriarchal institutions—precisely because it created an amplified valuation of youth in women and not men, and in turn early childbearing typically rendered women dependent on men and kin for resources. The gendered clock also implied accelerated social aging for women as compared to men insofar as social roles were tied to reproduction...It may be that the gendered clock in *Homo sapiens*, coupled with the cooperative breeding backdrop (which enables experienced 'helpers at the nest' to make up for the inexperienced young mother), accounts for what anthropologists see as the 'universal' gut-level valuation among humans of nubile young females—and the discount valuation of slightly older ones. (425)

While neither of these stories are implausible per se, they include significant, unsupported assumptions. For instance, Coe and Palmer do not provide any references to research that might support their assumption that early humans had clan names; that tribal names preceded cooperation with second and third cousins, and that it wasn't the other way around; that identification through names facilitates further cooperation. Likewise, Pridmore-Browne's account has a number of unsupported assumptions. For instance, it remains unclear how she knows older women in early human populations were undervalued compared to older men. What if early human populations were matrilocal? In such cultures, would women still be undervalued compared to older men? And if early populations were matrilocal, would women still have been so dependent on men? Alternatively, what if early humans lived in small groups. Would the "inexperienced young mother" still be valuable if she did not have helpers? Because Coe and Palmer and Pridmore-Brown's evolutionary stories do not offer any real evidence that the stories they are telling actually depict historical events, they are just stories. But like myths, stories are not legitimate kinds of data for explaining what happened in the past.

One way to avoid telling unsupported stories about the human ancestral past would be for FEP scholars to keep up to date with and engage scholarship from disciplines that study ancestral conditions and ways of life. For instance, FEP and EP scholars should regularly be checking and engaging research from especially paleoanthropology and human evolutionary biology. While it is true that behaviours themselves cannot be fossilized, there are all kinds of ways that ancestral human behaviours can be uncovered through physical evidence. Researchers from paleoanthropology and human evolutionary biology are regularly showcasing breakthroughs in their finds—discoveries they argue can shed light on how ancestral humans and other hominids once behaved. Importantly, these finds often challenge common beliefs about ancestral human

behaviour (e.g., see Cieri et al. 2014; Dyble et a. 2015, also see Gopnik 2014 for critical review), which is one good reason FEP and EP scholars should not rely on their general or tacit knowledge about early human life. Staying up to date on this research saves FEP scholars from having to reason hypothetically about phenomena that might already have been studied in physical terms.

5.3 Not enough of the right Data

In this section, I focus my critiques on FEP scholarship that does not necessarily offer the *wrong* kinds of data, but rather *not enough* of the right kinds of data for adaptationist hypotheses. For this I get into philosophical scholarship that outlines the standard types of data that are needed for successful adaptation explanations. I also discuss some problems FEP scholars have regarding the populations of humans they choose to study.

5.3.1 Data from too few Sources

Philosophers have criticized evolutionary psychologists for failing to provide any or enough of the right kind of data that are needed, according to standards in evolutionary biology, to support adaptationist claims. In his critique, Robert Richardson lays out the necessary kinds of information that would be needed to support a reliable explanation for an adaptation. He draws on Robert Brandon's (1990) list of five conditions of "adaption explanations." First, he says, the explanation must provide evidence of *selection* which would require, among other things, information about the character and extent of variation among ancestral forms, and differential survivorship and reproductive success among ancestral forms. Second, there needs to be information about the ancestral *ecological factors* that could explain the presence and the

strength of selection. Third, it needs to be shown that the supposed adaptive trait in question is *heritable*. Fourth, there must be information regarding *population structures* such as, aspects of the ancestral populations such as gene flow, mutation rates, population size, interbreeding. All of these aspects can affect the rate of evolution. Finally, there needs to be information about *trait polarity*, that is, whether the trait is primitive or derived. A trait that is inherited from distant ancestral lineages is not an adaptation, even if it appears to have function. For example, the sutures in the human skull have adaptive function as their flexibility facilitates a human baby's safe passage through the birth canal (Richardson 2007). However, they are not adaptations since they are present in birds and reptiles as well, and birds and reptiles do not have to pass through birth canals. Skull sutures, then, are a primitive trait that we have inherited from an ancestral line. Richardson holds this list of five conditions up to some adaptation explanations in evolutionary psychology and explains how they fall short in each category. At the end of his discussion, he criticizes more generally some evolutionary psychologists' tendency to rely on information regarding trait function as sufficient for adaptation explanations:

> The standards more typically embraced within the collection of views that is called "evolutionary psychology" are much less limiting [than those in evolutionary biology]. The approach is one that depends on what Tooby and Cosmides [two evolutionary psychologists] call "functional analysis." It begins with an analysis of what constitutes successful performance of a trait, relative to the presumed ancestral conditions in which it evolved. The central question is then taken to be whether some proposed design would, in those circumstances, have proven to be adaptive. My question is not whether some design might have been, or even would have been adaptive, but whether the right explanation for

> the presence of the proposed design, for example, human language and reasoning, is that it *is* an adaptation and how it should be explained. (137)

In summary, Richardson criticizes evolutionary psychologists for being ignorant of the kinds of information that are required to offer a robust adaptation explanation. Adaptations are multifaceted and so should be explained by investigating a variety of appropriate data sources. While adaptations often have functions, function alone is not a sufficient indicator that a feature is an adaptation.

Lloyd (1999; 2001; 2002) also argues that evolutionary psychologists do not provide the rights kinds of data to support their claims about human adaptations. For instance, in Lloyd (2001), she criticizes the types of data Thornhill and Palmer (2001) use to demonstrate that rape evolved in men as an adaptive reproductive strategy. She goes through a number of research avenues that Thornhill and Palmer would need to take to demonstrate this alleged evolutionary function of rape: inheritance studies to first establish that rape is a heritable trait; a comparison of the reproductive success of rapists and non-rapists to test whether rape is in fact adaptive; the use of comparative methods to see if the trait exists elsewhere in the lineage; an examination of past social structures, population sizes, migration rates, and material culture to determine if there really was a plausible set of circumstances that could lead to the evolution of rape as an adaptation. She explains, however, that Thornhill and Palmer fail to engage any of these adequately. For instance, she says, they try to get out of having to demonstrate that rape is a heritable trait by claiming that it is a "fixed" trait, i.e., that all men (and women) already have the genes that could lead to raping behaviour. But, she points out, this move is left undefended and does not exempt them from having to fulfill the other standards of evidence listed above. She notes that "nowhere in the book do the authors present evidence regarding either relevant details

of the past evolutionary environment or comparisons with our closest relatives. Nor do they discuss seriously the possibility that rape itself is not a single trait" (which would complicate its selection) (1542). Moreover, she goes on, most of the data Thornhill and Palmer offer concerns current statistics surrounding rape and rape victims, statistics that could lend support to the possibility that rape in humans is an advantageous reproductive strategy. But to this she says,

> This could be relevant evidence if they showed the relative reproductive success of rapists and nonrapists, but they do not. Thus, according to the usual evolutionary standards of evidence regarding demonstration that a trait is an adaptation, Thornhill and Palmer fail rather spectacularly. (1542)

So, for Lloyd as well, evolutionary psychologists fall short in offering the kinds of data needed to adequately explain a given feature as an adaptation. Like Richardson, she penalizes evolutionary psychologists for underestimating or being ignorant of the required standards surrounding adaptation explanations in evolutionary biology.

There are FEP researchers in my sample who neglect entirely to provide the kinds of information (that Richardson and Lloyd discuss) that are needed to support to adaptationist claims (i.e., Coe and Palmer 2013; Betzig 2013; Wilbur and Campbell 2013; Meredith 2013; Easterlin 2013; Sylwester and Powlowski 2011). A prevailing assumption across these articles is that noticeable patterns in current single-cultural populations that match predictions made by natural and sexual selection hypotheses are enough to support those hypotheses. The problem with this move, however, is that there are extraneous reasons why observations of current populations, and especially current populations of a single culture, might support adaptationist predictions. Ecological conditions, for example, could be triggering a particular expression of "phenotypic plasticity." Phenotypic plasticity is an organism's ability to alter its phenotype to

meet environmental demands. This includes changes that occur at the genetic level to changes that can result from learning. When phenotypic characteristics are plastic like this, it is very hard to know how they will be expressed from one period or environment to the next. Genetic drift, phylogenetic inertia, and epigenetic factors are other factors (other than selection) that can also cause phenotypic patterns in populations. Processes such as higher-level population structures (e.g., population density), socialization, development, and learning can also lead to phenotypic patterns but which are not genetically based, and so could not be the result of selection. Along these lines, recall Wilbur and Campbell's study that I talked about in Chapter 4. I pointed out how social and cultural factors easily explain the differences between men's and women's mate preferences involving humour that they found. Wilbur and Campbell, as well as the other researchers referenced at the start of this paragraph, therefore need to do a lot more work to show that such preferences are not socially contingent; that there were specific conditions in an ancestral past that led to the preferences being selected for. To do so, they would do well to test their predictions cross-culturally, and do what they could to determine if the "trait" they are interested in is heritable or has fitness benefits. I also recommend FEP scholars familiarize themselves with the standards in evolutionary biology that lay out what the appropriate criteria are for establishing an adaptation explanation. Robert Brandon's (1990) work as well as Lloyd's (1999; 2001; 2002) and Richardson's (2007) critiques are great places to start for becoming familiar with these standards.

5.3.2 Sample Unrepresentativeness

Evolutionary psychology occupies a challenging corner of the human sciences considering that many of the human subjects it studies (i.e., human ancestors from the Pleistocene) no longer

exist. This, of course, complicates finding a representative sample of human subjects to study—perhaps to the point that no sample at all from current human populations may really be appropriate. Nevertheless, there are better and worse samples that one can collect data from in order to try to answer questions about humans from a distant ancestral past. Leading evolutionary psychologists themselves caution other EP researchers about using appropriate samples. David Buss (2007), for example, encourages evolutionary psychologists to make use of data from a variety of human populations (including archeological records and data from hunter-gatherer societies). Along similar lines, Anne Campbell (2006) recognizes the importance of cross-cultural and developmental data.

Despite this stated conscientiousness about samples on the part of evolutionary psychologists, philosophers have pointed out many problems with sample representativeness in EP. Specific issues include problems with studying only undergraduate students (Buller 2006), problems with studying people from just one culture (Dupré 2001), and issues generalizing conclusions about non-humans to humans (Dupré 2001; Buller 2006; Richardson 2007; Meynell 2012; Rosser 2003). The relative homogeneity of undergraduate students makes them unrepresentative of the broader population. Undergraduates, are often similar in age, have similar values and interests (especially if the students being measured all have a similar major like psychology), and often have similar socioeconomic statuses and backgrounds. Samples from one culture are also very limited in the context of evolutionary research. Because human behaviour and cognition are so highly influenced by social and cultural factors, one can't be sure that patterns within a culture are evolutionarily caused. Cross-cultural comparisons are needed to tease out the effects of a culture. Finally, regarding the study of non-human animals, some EP researchers have been accused of "cherry picking" species based on how well they meet the

researchers' predictions about alleged universal evolutionary causes. For instance, feminist scholars have critiqued evolutionary researchers for choosing to focus on chimpanzees and baboons for their comparative research on male competition and control (Lloyd 2001). Chimpanzees and baboons have especially male dominated social structures relative to other primates so are not representative of primates as a whole. Making universal claims about primates, therefore, based on these species, sends the false message that primate males (including humans) in general are highly aggressive, competitive and controlling of females. The limitations surrounding samples with these three types of issues are well known in psychology and comparative studies. Researchers are expected to control as best they can for these limitations or to at least acknowledge them in their discussions.

Samples in FEP are vulnerable to these same criticisms. Almost a third of the articles from my sample literature have acute limitations with regard to sample representativeness. Easterlin (2013), for example, derives the data for her research from three fictional characters from the story *Jane Eyre*. She uses the characters from this story to test evolutionary psychological predictions regarding male control of and proprietary attitudes toward women in the late nineteenth century. However, any information about these characters should not be taken as representative of individuals from that time period, or any time period. The characters were invented by the author. The character's attitudes, beliefs, and behaviours may deviate significantly from non-fictional persons of the time period of focus.

Other FEP researchers from my sample are also incautious regarding how liberally they generalize their findings from non-representative samples (Betzig 2013; Coe and Palmer 2013; Johow et al. 2013; Seaman 2013; Sylwester and Pawlowski 2011; Wilbur and Campbell 2013). For example, referring to women as the "empresses of the kitchen," Kathryn Coe and Craig

Palmer (2013) claim that selection on women's instructional cooking activities in the Stone Age enabled the development of human traditions, which they suggest are adaptive. Their evidence for the ubiquity of cooking as a woman's role is taken from data from 39 African cultures dating back a few decades (HRAF 2016). However, why these particular cultures were chosen is not discussed. Perhaps taking data from a sample in a Western culture would be problematic given the commonality of men's roles as cooks, both at home and especially professionally (the vast majority of cooks/chefs in the restaurant industry are men). But this is a problem Coe and Palmer should contend with not sidestep by focusing on a non-random selection of cultures where men happen to do little cooking. Along similar lines, perhaps Coe and Palmer chose to study these African cultures because they think they are closer in kind to ancestral cultures, for instance because they are hunter-gatherer cultures, or because there is evidence to suggest ancestral cultures had strict divisions of labour that required women to do all the cooking. But Coe and Palmer do not establish either of these. They do not specify if the cultures in their sample are hunter-gatherer, and it is unlikely that they are since very few hunter-gatherer societies actually still exist. Nor do they provide any evidence (e.g., paleoanthropological) that could indicate that women predominantly cooked in ancestral populations. To guard against charges of cherry picking their data (and racism),[19] Coe and Palmer should offer a rationale for why they have chosen to study the particular populations they focus on.

[19] Because Coe and Palmer don't explain why they chose to use data from African cultures, they leave it open to interpretation that perhaps they think African cultures are somehow more primitive, and this is why data from African cultures are especially relevant to the study of human evolution. But this is deeply racist. African cultures have been around for just as long—if not *longer*—than other cultures throughout the world. This means they have

These other FEP scholars have issues with their sample representativeness as well. Betzig (2013), Seaman (2013), Sylwester and Pawlowski (2011), and Wilbur and Campbell (2013) take their samples from just a single culture. Betzig (2013), Easterlin (2013), Sylwester and Pawlowski (2011) take their data from just a subsection of a single culture—i.e., undergraduate students from an American university, the queens of England, and a few characters from the story *Jane Eyre*, respectively. In all of these cases, the authors do little to no work to defend why they think data from these samples is representative of humans at large.

I recommend FEP scholars pay more attention to the representativeness of their samples. If EP is as evolutionary psychologists say, a "science of human nature," then studies conducted on undergraduate students, the queens of England, or characters from a fictitious novel should be a very last resort and their results discussed with the utmost caution. As I mentioned earlier, because EP makes claims about all current and past humans, the research subject pool of EP is the largest in the human sciences. In principle, then, evolutionary psychologists, including FEP scholars, should have some of the most stringent criteria for where they get their human data from. Cross cultural samples, inheritance and fitness studies, and references to historical populations when possible should be the norm.

Conclusion

This chapter accomplished two things. I developed a novel philosophical critique for operationalization practices in human evolutionary research, and offered a critical assessment of the kinds of data FEP scholars use to support their adaptationist hypotheses. Regarding the first, I

been subject to as much, if not more, evolutionary and cultural change. There is no reason to think African cultures are more similar to ancestral cultures, especially if they are not even hunter-gatherer.

showed that FEP scholars are often unsuccessful at establishing an adequate link between the evolutionary phenomena they purport to be assessing and the proxy phenomenon they are measuring. I pointed out that it is crucial that FEP scholars, but also other human evolutionary scholars, be vigilant about this step in their research. Spending more time establishing and explaining the link might catch inadequate links and therefore save them from collecting data that is irrelevant to the evolutionary phenomenon they are interested in. Regarding the second, I demonstrated that past philosophical critiques of the kinds of data evolutionary psychologists use are still relevant to FEP. This is concerning. Not adhering to standards for adaptation explanations, telling just-so stories, and conducting their research on strikingly unrepresentative samples are basic mistakes that have been pointed out to evolutionary psychologists time and again. Mistakes like these indicate to many that, as an institution, EP has unacceptably low standards of practice. If FEP scholars would like to see their discipline rise above this reputation, and to be recognized for their successful practices, they would do well to learn from the critiques of EP that could help fix such mistakes. The recommendations I have offered in this chapter and the last would be a good place for them to start.

EP and FEP are not just the business of their practitioners. Like any publicly available and publicly-funded science, EP is located within a network of interconnected epistemic communities. The knowledge it produces affects and is affected by many other epistemic bodies. What is more, EP is a human science that makes explicit attempts to inform policy and to inform how people understand one another. The quality of its research therefore has an important bearing both on other scientific communities as well as social ones. In the next chapter, I turn my focus to the social impact of and values in FEP. FEP research is both implicitly and explicitly political. It is implicitly political because it makes claims about the "nature" of women and thus

demarcates who counts as a woman, and what the "normal" characteristics of women are. It is, however, also explicitly political as it calls itself feminist and purports to promote gender equality. My next set of analyses look closely at the political dimensions of FEP, its values and social impact. I offer further recommendations for how FEP scholars might better scrutinize their values and navigate the social impact of their research.

Chapter 6
Critical Analysis of FEP Values

6.1 Introduction

This chapter engages the second prong of the social-dimensional approach to science criticism that I developed in Chapter 2 and Weaver (forthcoming). In this chapter, I consider the problematic ways in which certain social values lend to the production of some FEP knowledge claims and risk harm to women[20] (social impact). I reveal how these social values guide FEP research in terms of how FEP scholars understand the relationship between feminism and evolutionary psychology, their topics of study, and how they interpret data. I argue these values (1) compromise the quality of FEP research, and (2) jeopardize FEP's feminist aims. The chapter is divided into two parts. In the first part, I identify and discuss the consequences of FEP values of Authority. Recall that to value Authority is to value one's own or one's group's positions as positions of far-reaching epistemic influence. The scope of one's influence ought to be bounded by one's resources (tools, data, methods, theory, etc.), expertise, knowledge, and professional/social position. Problems can arise if one either underestimates (wimpiness, unused expertise) or over estimates (imperialism, false confidence) the scope of their influence. In this chapter, I argue that FEP has problems with Authority because (a) there is a theme in FEP scholarship in which they seek to advise feminists on how to conduct their research but lack the knowledge and expertise to do so, and (b) FEP scholars depend on an unjustified expectation of the unity of science to argue feminists should incorporate EP research into their work. I recommend FEP scholars take a more pluralistic approach to the study and understanding of feminist issues.

[20] And harm to persons from other marginalized groups, though my main focus is on women in this chapter.

In the second part of this chapter, I identify and discuss the consequences of FEP values of Social Order. To value Social Order is to value an organization of people into social roles that are conducive to a certain end state of society. One can have progressive values of Social Order such that the social roles you advocate for are conducive to an egalitarian end state. However, one can also have non-progressive or harmful values of Social Order if the social roles you advocate for promote an end state of society in which certain groups of persons are oppressed or otherwise systematically mistreated (e.g., racial or class hierarchies, patriarchy). In this section of the chapter, I uncover in FEP values of Social Order that promote social roles that are conducive to a patriarchal society. These values appear to be guiding FEP choices of study, focus, and interpretations of data. FEP values of Social Order are harmful but also run counter to FEP's feminist aims. I recommend FEP scholars incorporate structures into their research that help them scrutinize their values of Social Order.

6.2 Part 1: Authority

Recall from Chapter 2, FEP scholars aim to supplement extant feminist research with EP research so that a broader understanding of women more generally might be gained. To FEP scholars, this broader understanding of women can help in social efforts to eliminate gender inequality since such knowledge could be useful for finding solutions to the social and political problems women face. For example, remember Easterlin's (2013) suggestion that understanding sex inequality from an EP perspective provides a more accurate picture of how differences between the sexes promote inequality and why this is harmful. Or Buss and Schmitt's (2011) argument that a consideration of EP accounts of rape "would aid intervention efforts to reduce the frequency of sexual assault by focusing attention on the subset of males most prone to rape and to commit serial rapes" (779). Also recall Singh and Singh's (2011) EP account of cultural

expectations of beauty, that the waist-to-hip ratio that men find most attractive is also a signal of health. Singh and Singh suggest understanding this link between beauty and health can be empowering for women:

> If women were made aware of the link between health and beauty, such awareness could potentially minimize the oppressive search for becoming beautiful. Instead of seeing beauty as a tool of oppression, understanding how health and beauty overlap can, in fact, be empowering to women. (729)

I argue, however, that FEP ideas for how to supplement extant feminist research with EP are imperialistic and so represent inappropriate values of Authority. Particularly, I argue that (a) there is a theme in FEP scholarship in which FEP scholars offer advice to feminists but lack adequate knowledge and expertise to do so. I also show that (b) the argument FEP scholars put forward (i.e., the "amalgamationist argument," see below) for why feminists should integrate their work with EP depends on an unjustified expectation of the unity of science. In consequence, they are mistaken to extend the epistemic scope of EP as far as they do into other feminist domains. These problems with FEP values of Authority risk negative social impact as they jeopardize the feminist aim in FEP to help resolve inequality issues women face. FEP scholars are not experts on feminism, broadly speaking. The perspectives on women and how to resolve women's issues that they have to offer are narrow and limited. A widespread adoption of their perspectives across feminisms would hinder rather than help the very pluralistic and far-reaching aims feminists have.

6.2.1 The Amalgamationist Argument

Some FEP scholars purport that, in order for feminists to be successful in their social justice projects, they need to understand the phenomena they study from an EP perspective. One can see this in FEP researchers' emphasis on what they see to be the importance of understanding biological phenomena in terms of their "ultimate" and "proximate" causes. The model of ultimate and proximate causation, as it is characterized in FEP, distinguishes between ultimate, or evolutionary causes of a trait or behaviour, and the more immediate or proximate causes of that same trait or behaviour that are apparent in an organism's life time (Fisher et al. 2013; Reiber 2013; Vandermassen 2011).[21] FEP scholars explain that studying proximate causes is to answer the "how" questions about organisms. For instance, how do birds fly south without maps? In this area of study, such answers would be found by studying things like genes, hormones, brain mechanisms, individual life experiences, and sociocultural influences. On the other hand, one might wonder *why* birds fly south, or why they have the genes, hormones, brain mechanisms, etc. to begin with that facilitate migration behaviour. These "why" questions, according to evolutionary psychologists, are attributable to the ultimate level of causality, the level of evolutionary explanation. In this area of study, such answers would be found by studying things like natural and sexual selection. Parsing the causes of phenomena in this way leads FEP researchers to insist that because biological processes have both types of causes, they ought to be studied from both proximate and ultimate research. For instance, Buss and Schmitt (2011) argue that "scientific progress" can only be "facilitated by the recognition that all social scientists should make their underlying assumptions explicit, and that all psychological hypotheses are

[21] The ultimate/proximate distinction is commonly attributed to Ernst Mayr's characterization (e.g., see Mayr 1993). While it is arguable that the FEP scholars I cite in this discussion do not capture the distinction as Mayr would have it, that is not my concern here. I am simply bringing up the distinction as FEP scholars lay it out since this is a feature of the amalgamationist argument.

implicitly or explicitly evolutionary in nature" (782). Reiber (2013) echoes something similar when he insists that an understanding of women's health "will not be complete until the evolutionary level is also mastered, because evolutionary biology is the primary science underlying all of medicine" 208). FEP scholars who make this argument reason that because most feminist research is proximate research (as feminists are often interested in mental, behavioural, and social processes that are relevant to social issues), they ought to integrate their studies with EP research (i.e., research that studies ultimate causes of human behaviour and psychology). That way feminists can offer a fuller causal picture of the phenomena they study. Let's call this the amalgamationist argument for the integration of feminism with evolutionary psychology (I take these arguments from Buss and Schmitt 2011; Easterlin 2013; Seaman 2013; Fisher et al. 2013; Reiber 2013; Vandermassen 2011).

Importantly, these particular amalgamationist arguments require feminists to act. Feminist scholars are chastised for their resistance to evolutionary accounts of the phenomena they study, and are asked to change their research strategies such that they are more open to, or incorporate, EP research, theories, or methods. For example, Easterlin (2013) says,

> Feminism's animus to evolutionary psychology has been particularly pronounced in the academic humanities, where a prevailing constructionist viewpoint has dominated since the advent of postructuralism, generally dated to Jaques Derrida's 1968 landmark lecture "Structure, Sign, and Play in the Discourse of the Human Sciences" (Derrida, 1968). The dogmatic insistence on constructionism, the perspective that differences are socially produced rather than the result of innate predispositions, assumes a misguided correlation between *difference* and *inequality*. …In this chapter, I claim, first, that feminism—a movement that eschews social and other forms of bias against women, seeking to bring

its causes to consciousness and thus lead to the elimination of sexual discrimination—ignores research on evolved sex differences to its peril. (Easterlin 2013, 390-391)

Seaman (2013) says something similar. She claims that in response to one evolutionary psychology account of rape, despite the researcher's attempts to explain why his account did not imply genetic determinism, many feminists were nonetheless "outraged by the suggestion that rape was not a wholly 'socially constructed' behaviour and that biology might be invoked to justify male sexual aggression" (409). To this Seaman responds,

> But a refusal to engage with—or even to acknowledge as legitimate—the vibrant and diverse fields that make up contemporary evolutionary studies risks throwing out the genuine insights with the invidious stereotypes. (Seaman 2013, 409)

Vandermassen (2011) also warns feminists that their close-mindedness about EP is to their disadvantage. On the feminist reception of *A Natural History of Rape*, she says,

> The book's reception attested to a state of affairs condemned strongly in the book itself: the strong hostility of many feminists and social scientists to the possibility that biology or the evolutionary sciences might contribute to our understanding of the human mind and behavior. As I have acknowledged elsewhere, this hostility is to some extent understandable, given the history of male bias in science and given the danger that appeals to biology might be used as justification for repressive policies, as they often have. Yet, as I have argued, feminism will have to forsake this hostility if it wants to retain its intellectual credibility. The evidence is overwhelming that we are not born as blank slates or psychosexually neutral. (Vandermassen 2011, 733)

Taken together, these FEP scholars are offering advice to feminists regarding where they think feminists go wrong. Feminists are criticized for being "dogmatic," "hostile," and negligent

for ignoring EP insights about phenomena relevant to feminist research. The implication is that they need to change and become more open to EP accounts of the specific phenomena (e.g., sex differences, rape). In what's next, however, I argue that this advice is motivated by a misunderstanding of feminist resistance to EP, and that the alleged problems with feminism they point out are not problems at all. Their advice to feminists therefore lacks appropriate expertise and is misguided.

6.2.2 Misappropriated Feminist Expertise

To begin, it is unclear who the target feminists are that FEP scholars take issue with, so it is impossible to know exactly what theory or theories they are attacking. Talking about "the feminists" or "feminism" so broadly, and without citing anyone, is a recipe for a straw person argument. FEP scholars are claiming to be experts on feminism and its contents, but give their reader no opportunity to verify their claims. As such, they have free rein to characterize feminism in a way that suits their argument. And, indeed, they do this. For example, according Vandermassen (2011), feminists are "hostile" to the idea that "biology or the evolutionary sciences might contribute to our understanding of the human mind and behavior" (733; also see Seaman 2013, 409). To Fisher et al. (2013), feminists are not "sophisticated" because they assume feminists embrace a "nature versus nurture" divide (Fisher et al. 2013, 10). And to Easterlin (2013) feminists hold a "dogmatic insistence on constructionism, the perspective that differences are socially produced rather than the result of innate predispositions (390). Characterizing feminism in this way makes it easy to criticize.

FEP scholars are not off base when they indicate that feminists have been very critical of EP. Indeed, many feminists have been so openly (e.g., Dupré 2001, 2012; Drea and Wallen

2003; Fausto-Sterling 2000; Fausto-Sterling et al. 1997; Gannon 2002; Lloyd 2001, 2003; Lloyd and Feldman 2002; Martin 2003; Meynell 2012; Rose 2000; Rosser 1997; Sork 1997; also see discussions in Fehr 2011). These scholars have conducted extensive critical work on the political dimensions of EP, but have also combed through EP studies to see how they measure up to the standards of evolutionary biology. Many of them have applied rigorous feminist analyses to the inappropriate assumptions evolutionary psychologists make, and have offered superior methods, theory, and data that evolutionary psychologists can and should make use of. Oddly, however, FEP scholars do not cite these works. Yet if they are interested in undermining feminist resistance to EP, these are precisely the arguments FEP scholars should be concerned about. The feminist authors of these works articulate very specifically why they are resistant to EP claims. An analysis of these feminist papers shows that the characterization of *the* feminist stance described by FEP scholars is mistaken. None of these feminists indicate any "hostility" toward biology or even the evolutionary sciences. (Note, too, that EP is not one and the same with biology or the evolutionary sciences. Expressing hostility towards EP should not be equated with expressing hostility towards biology or the evolutionary sciences.) In fact, most of these feminists are well versed in biological and evolutionary research on a variety of relevant topics to EP. They even recommend to evolutionary psychologists how they might make better use of or expand their knowledge of, specifically, evolutionary biology (see especially Dupré 2001; 2012; Fausto-Sterling et al. 1997; Gannon 2002; Lloyd 2001; 2003). These scholars are also acutely aware of the problems with the "nature/nurture" divide and make efforts to explain why evolutionary psychologists are wrong to assume social scientists, including feminist social scientists, are stuck in this debate (e.g., Rose 2000).

Altogether, FEP scholars do not demonstrate sufficient familiarity and engagement with relevant feminist scholarship to credibly critique feminist projects. As a result, their valuation of their own Authority in the context of offering feminists advice on how to conduct their research is inappropriate.

6.2.3 Exaggeration of EP's Scope and Usefulness

Returning to the amalgamationist argument, in this section I show how it depends on an unjustified expectation of the unity of science. In consequence, FEP scholars are mistaken to extend the epistemic scope of EP as far as they do into other feminist domains.

Setting aside FEP mischaracterization of feminism and failure to address feminist concerns with EP, a question still remains: are FEP scholars right that incorporating EP perspectives would benefit feminism in the ways they claim? To answer this, let's look in a little more detail at the benefits FEP scholars think EP offers feminism. The underlying claim in the amalgamationist argument is the claim that feminism, a domain of so-called proximate research, is deeply lacking evolutionary psychological, or ultimate, explanations for the phenomena studied. Embedded in this claim, is the assumption that every, or at least the vast majority of biological or behavioural phenomena that feminists are interested in has a so-called ultimate cause. Buss and Schmitt (2011) make this assumption explicit:

> More generally, it is important to recognize that, at some level, all psychological hypotheses are implicitly or explicitly evolutionary psychological hypotheses. First, no other known causal process has been discovered, other than evolution by selection, that is capable in principle of producing whatever complex psychological mechanisms humans possess. Second, those who do not explicitly invoke evolutionary psychology implicitly

> assume, unless they are creationists, that selection has fashioned evolved domain-general learning mechanisms (e.g., those that are capable of adopting whatever social role one's culture hands out). One virtue of evolutionary psychology is that these underlying assumptions are made explicit. Scientific progress will be facilitated by the recognition that all social scientists should make their underlying assumptions explicit, and that all psychological hypotheses are implicitly or explicitly evolutionary in nature. (Buss and Schmitt 2011, 782)

To these FEP scholars, human behaviours and cognition *just are* caused by both proximate and ultimate (i.e., "evolution by selection") causes, and that to fully explain behaviours and cognition, one must be able to give an amalgamated account of both types of causes.

To explain what is wrong with the amalgamationist claim, I will return to my discussion in Chapter 3 of John Dupré's (2001; 2012) critique of reductionistic scientific imperialism. Recall, Dupré problematizes the pervasive monistic assumption that explanations of phenomena across scientific disciplines are organized along hierarchical levels such that phenomena at higher levels can be explained in terms of phenomena at lower levels. So, for example, this would be to assume that neuroscience and psychology can be unified because the human behaviours psychologists study can be reduced to the neurological processes that are the subject of neuroscience. Part and parcel with assumptions of such scientific unity is the assumption that explanations at the lower levels are superior—i.e., they explain more about a given phenomena—to higher-level explanations. According to Dupré (2012), evolutionary psychologists are keen to emphasize the superiority of their discipline given where in the hierarchy of scientific explanations they take their own explanations to occupy. For example, he (2001, 2012) cites evolutionary psychologists Barkow et al. (1995) doing just this. Propounding

a version of physical reductionism, these evolutionary psychologists emphasize the importance of scientific explanations that appeal to the internal structural properties of phenomena, as opposed to explanations that highlight context and environment. This is meant to epitomize EP (a science of genetics and "mind modules") as a hyper-scientific account of human behaviour, over and above the more traditional human behavioural disciplines that must contend with "chaotic" phenomena like culture (as cited in Dupré 2001, p. 73).

In Chapter 3 I explained how Dupré's discussion of the ideology of reductionistic scientific unity and how evolutionary psychologists can gain from this ideology sheds valuable light on the ways in which inappropriate values of Authority permeate EP research. By assuming a reductionistic unity of science and claiming to occupy a crucial part of this unity, evolutionary psychologists enhance the epistemic scope of their discipline. This is then used to discredit other disciplines' theories of human behaviour (especially those disciplines that do not appeal to the supremacy of endogenous causes) that they claim do not fit as tightly within the science unity that evolutionary psychology allegedly does.

FEP scholars appeal to a very similar type of reductionistic scientific unity. To them, higher level cultural and social phenomena can and even must also be explained in terms of their lower level, evolved psychological mechanisms. FEP scholars then use this unity requirement to discredit certain feminist explanations. They chastise feminists who resist the reduction of social and cultural phenomena to evolved mechanisms, claiming such feminists are out of touch with science and so are doing their discipline a disservice.

FEP's use of the Authority of scientific unity, however, is problematic given the untenability of this kind of scientific unity. There is much philosophical scholarship on the problems associated with efforts to unify theories or the phenomenological contents of theories

across disciplines that study different levels of organization (Bechtel 2007; Cartwright 1999; Dupré 1995; Fehr 2006; Hochstein 2016; Longino 2006; 2013; Oppenheim and Putnam 1958). For example, Bechtel (2007), who argues against the idea that psychological hypotheses are always reducible to neuroscientific hypotheses, points out that the capabilities of higher level psychological mechanisms cannot always be predicted based on knowledge of lower level mechanisms. He says,

> Especially when organization [of mechanisms across levels] is non-linear, it can enable mechanisms to generate phenomena that are quite surprising given the operations of the components taken in isolation. Such organization must be discovered—it cannot simply be derived from knowledge of lower-level parts and their operations. (172).

He uses this to argue that higher level studies therefore make autonomous, as opposed to reducible, contributions to the understanding of psychological mechanisms. Eric Hochstein (2016) makes a different but related point about the irreducibility of psychology to neuroscience. He notes that scientists in both disciplines necessarily need to idealize the phenomena they study, but that the idealizations required for the higher-level discipline often render the phenomena unintelligible in the context of the lower-level discipline. He uses the example of psychological theories that make reference to emotions and specific emotions like "anger." He notes that some have argued that because emotions under the same category (e.g., anger) often subsume disparate and unrelated neurological mechanisms and processes, they therefore "have no place in a correct theory of the mechanisms responsible for behavior" (e.g., Griffiths 1997, 2004). However, Hochstein explains,

> if we think of these emotion categories as idealizations, as simplified categories used to mitigate the complexity of the system in order to better characterize its overall

> behavioural capabilities, then there is evidence that psychological theories which employ such categories played an essential role in our study of cognitive behaviours. (139-140)

If psychological theories always had to appeal to neuroscientific theories as a basis for characterizing the cognitive behaviours, they would get bogged down by having to account for the causal influences of those theories *plus* the causal influences at the cognitive level. So, sometimes it is better that psychological theories posit idealized states and causes that, albeit may not have a place in neuroscientific theories, can mitigate complexity and allow for the creation of useful testable hypotheses about the behavioural capacities and patterns of the cognitive system as a whole.

Carla Fehr (2006) makes similar points as Hochstein but in the context of the evolutionary study of sex. She discusses the varying theories explaining the evolution of sex and addresses the common expectation that the theories will one day be amalgamated into a unified account. She argues that such a unified account is not possible "without loss of content or explanatory information" (168). She draws on Helen Longino (2002) who argues that various assumptions made in different epistemological communities create specific

> intellectual architecture[s] on which explanations hang. These include substantive assumptions about what the world is constituted of and the processes in the world that need to be explained, as well as methodological assumptions about how knowledge is created. (as cited in Fehr 2006, 177)

Fehr applies this insight to the case of the evolutionary study of sex and reveals incommensurate sets of theoretical and methodological assumptions across the different epistemological communities that study sex. For instance, she talks about how the different communities have different assumptions about what sex is (e.g., meiosis, outcrossing, fertilization). They have

different assumptions about which fundamental processes render sex intelligible (e.g., biochemical processes vs. selection processes). And they rely on different kinds of abstract individuals (e.g., models of cellular processes vs. models of ecology and population) to study sex. Here Fehr notes how the different models in the accounts must necessarily abstract away many of the phenomena studied by the other accounts in order to arrive at the idealized individuals that they do. It becomes a serious question, then, how these accounts could be unified if the different phenomena that the accounts study actually get in the way of each other.

These philosophical insights shed light on the complexities of knowledge production within different epistemic domains. How knowledge is produced in one context is not a good indication of how knowledge is or ought to be produced in another. FEP scholars oversimplify the knowledge producing practices in different disciplines when they insist that feminists amalgamate their production practices with FEP's. Some feminists may, depending on what they are studying, have to render evolutionary processes invisible or operationalize the phenomena they study differently than evolutionary psychologists do in order to make the phenomena they study intelligible. For instance, feminists who are interested in studying variation within groups (e.g., sex, race) necessarily cannot abstract variation away in the same way evolutionary psychologists do. Evolutionary psychologists make claims about universal adaptations, and so often ignore variation within the groups of people they study. Also, how evolutionary psychologists define or parse the groups they study render phenomena that some feminists study unintelligible. Evolutionary psychologists, for example, define sex according to parental investment. Males are those individuals with cheap, small gametes, while females are those with the larger, more costly gametes. But this is just one way to define sex. There are other physiological and psychological features that are related to sex, but not all of these correlate with

gamete size. In other words, these additional features of sex complicate the dichotomy that characterizes evolutionary psychology sex research. Yet many feminists may need this more "messy" characterization of sex because such features affect the lived experiences of those people feminist scholars study.

The points just made apply most obviously to feminist evolutionary psychologists who have particularly broad sweeping amalgamationist views, like Buss and Easterlin. For Buss and Easterlin, there are almost no corners of the study of human psychology and behaviour that are beyond the purview of EP. So, to them, all areas of feminism that touch on the study of human behaviour and psychology ought to incorporate EP hypotheses. The anti-unity of science arguments above undermine this strong type of amalgamationism because they undermine the idea that theories across disciplines and their contents can be amalgamated wholesale. But what about those feminist evolutionary psychologists who argue for amalgamation in very specific contexts (e.g., Vandermassen on rape)? Most philosophers concede that at least some theory interdependence/integration across disciplines is possible, desirable even (Dupré 1993; Fehr 2006; Hochstein 2016; Wylie 2002). So, it remains to be asked, could the FEP scholars who propose amalgamation between "feminism" and EP in highly specific contexts be justified?

No, I think even in the specific cases, for instance where FEP scholars have done some leg work to explain why EP is relevant to some specific topic X, FEP scholars are still unjustified to demand amalgamation from feminists who also study X. I argue this on the grounds that feminist evolutionary psychologists still might not know what X looks like from the other discipline's perspective. Philosophical criticisms of EP research on rape make this point salient. Feminist and non-feminist philosophers have criticized EP theories of rape on the basis that EP theories of rape have an overly-narrow definition of rape (Lloyd 2001), consider rape of only

women by men (Koss 2003; Lloyd 2001), and do not consider the many different motivations behind rape (Koss 2003; Travis 2003). What these criticisms show is that different disciplines (e.g., philosophy and EP) categorize and attend to this phenomenon differently. What might seem relevant or important to a topic in one discipline might be unintelligible, impractical, irrelevant, or banal to the study of that same topic in another discipline (recall Fehr's analysis of the different theories of the evolution of sex). Hence, in principle, even in highly specific areas of study, FEP scholars should not assume their theories will "fill in the gaps," or what have you, in the theories of other disciplines. Combining theories across disciplines is a highly complex affair that, to be done right, should require intercommunication and collaboration between the various disciplines involved.

In review so far, FEP scholars' valuation of their Authority on the shortcomings with "feminism" and the scope and usefulness of EP are overextended. FEP scholars have not demonstrated knowledge of feminist concerns with EP. They are therefore in no position to chastise feminist researchers, or to offer feminists advice on how they ought to better their disciplines. FEP scholars are also mistaken to insist that feminism and EP be broadly integrated, or even integrated in specific topic areas. FEP scholars do not have adequate access to the epistemological platforms that various feminisms work from, so they cannot say with any confidence that EP approaches and methods would complement them.

These problems with FEP imperialist Authority jeopardize the feminist aim in FEP to help resolve inequality issues women face. The various feminisms that tackle social issues related to gender inequality are many, and for good reason. There are over 3 billion women in this world who vary by age, race, wealth, sexual orientation, physiology, lived experiences, personality, education, and so on. How gender inequality affects women is in many respects

going to vary according to these differences. This is why we need so many feminisms. No one feminism can possibly understand "women," whatever this term entails, or their social plights when the complexity of the class of women is fully appreciated. FEP scholars demonstrate ignorance of this complexity when they insist feminists need to take an EP perspective, and this ignorance is damaging. Homogenizing feminism in the way FEP scholars envision would narrow feminist focus and limit the ways in which feminists try to understand and solve social problems. For instance, imagine someone working in trans feminism trying to work through social issues related to trans individuals but being required to work with the definition of "sex" that evolutionary psychologists use. This definition would render their subject group invisible or problematic. This would be catastrophic. If FEP scholars are serious about contributing to eliminating gender inequality, it is important that they do not lose sight of the importance of pluralism in feminism. Being pluralistic about the study of feminist issues, and understanding the limits of their own discipline, will help FEP scholars embrace the complexities of the issues they study and are concerned about. To better understand the limits of their discipline, I recommend FEP scholars begin by acquainting themselves with the feminist critiques of EP that I cited above. I also suggest FEP scholars consider pursuing collaborative projects, or organizing joint scholarly meetings with feminists from areas that are critical to EP. These ventures could lead to mutually open integrative projects and, at the same time, fruitfully elucidate points of compatibility/incompatibility between FEP and other feminist disciplines.

6.3 Part 2: Social Order

A patriarchal ordering of society would have it that men are authority figures and public leaders, and women are subjugated to men and domestic caregivers. People who live in societies that

value this kind of ordering are taught to believe that such ordering is natural and necessary for successful social life. Scientists are not exempt from holding such beliefs, and may hold them without knowing. These beliefs may influence what they choose to study (e.g., sex differences as opposed to similarities), what they focus on in their studies (e.g., cherry-picked data that confirms the naturalness of gender or class norms), and what they see in their data (e.g., seeing confirmation where there is none). In this section, I reveal some instances in FEP research in which a patriarchal ordering of sex roles and stereotypes appear to be guiding FEP choices of focus and/or data interpretations. In the first section, I discuss instances in which a valuation of the Social Order that men are social leaders and women are caretakers leads FEP scholars to hold implicit but unsupported assumptions about women's evolutionary role as nurturers, women's dependence on men, and women's social and physical inferiority to men. The particular values relating to Social Order that are built into FEP research compromise the quality of FEP study and impede FEP aims to produce literature that can contribute a positive social impact by lending to the elimination gender inequality. Values of Social Order that see women as followers of men, rather than as their equals or even as leaders, lead to scholarship that *reinforces* beliefs about gender inequality. If FEP scholars are serious about using their research to help resolve issues of gender inequality, they need to be made aware of the values that are sabotaging this goal, and make efforts to adopt new ones.

6.3.1 Women as Nurturers

FEP scholars are apt to point out that nurturing is important for human survival. Human infants are helpless due to their premature birth and require a lot of nutrients. FEP scholars therefore postulate that women's ability to nurture infants is an important target for natural selection. The

female role as nurturer is focused on extensively in FEP (i.e., 13 out of 29 papers in my sample). While women have likely contributed to human evolution in many other ways, FEP scholars predominantly choose to study women as nurturers. Note, however, that this focus on nurturing is not guided by any known facts about human evolution as the study of women's contributions to evolution has only just begun. Hence, FEP scholars can only assume that women's main role in evolution is nurturance. This assumption leads FEP scholars to be hyper-focused on studying women as nurturers. This, in turn, leads to more data on women as nurturers. And the overabundance of data on women's role as nurturers artificially confirms the assumption in FEP that women are primarily nurturers.

Social values surrounding women's role as nurturers fill in gaps between actual evidence provided and conclusions drawn. In the instances below, FEP scholars use problematic data, or interpret their data in problematic ways, in order to make claims about the evolutionary significance of women's roles as mothers.

In more than one instance, Nicole Cameron and Justin Garcia (2013) attempt to present evidence for "maternal effect" in humans. However, looking at the evidence they actually offer, it seems they frequently conflate maternal effect with what would better be characterized as parental effect. Consider these two examples:

> Mothers are generally the principal caregiver for their children, and the quality of the mother-child dyad influences the organization of secure-based attachment and behavior in the infant (Coall, & Chisholm, 2003) (143)

> Maternal investment during childhood often translates to the amount of time spent supervising offspring. For instance, in most developed countries accidental injury is the

> primary cause of death in children over 1 year of age (Belsky et al., 2010). Child supervision is a critical determinant of child injury in both preschool age children (Bredy, Humpartzoomian, Cain, & Meaney, 2003) and those 7 – 10 years old (Fish et al., 2004). Mothers reporting less supervision raise children who sustain more injuries.

In both excerpts the authors make the claim that specifically *maternal* care has certain direct effects on child well being and behaviour, however the actual evidence they present rather supports that *parental* care has these effects. In the first excerpt, the paper cited at the end (Coall, & Chisholm, 2003) makes no claims about the importance of the mother-child dyad on secure-based attachment. All discussions about attachment in Coall and Chisholm (2003) are unspecific to mothers. In the second excerpt, Cameron and Garcia are using data about child supervision that is, again, not mother-specific to support the claim that *maternal* investment determines amount of injuries in children. I should point out as well that the latter two references in this second excerpt actually say nothing about the significance of parental supervision on child injuries. Bredy et al. (2003) is actually a study on maternal effects in rats and does not discuss human (or rat) parental supervision in any way. Fish et al. (2004) do discuss the significance of rat studies (and studies from other species including some plants) for thinking about human parenting, but they in no way address the topic of parental supervision. A charitable interpretation is that both of these are referencing mistakes. In any case, the final sentence of the excerpt, "Mothers reporting less supervision raise children who sustain more injuries," is not at all supported by the evidence the authors provide.

There are similar issues with Elizabeth Oberzaucher's (2013) uses of neuroscience data to support claims about women's brains being "more emotional" than men's. Oberzaucher promotes the hypothesis that women evolved to be more emotional than men as high

emotionality is an asset for caretaking and getting others to help with caretaking. Emotionality leads to sociality which helps women establish long-lasting relationships with mates and others who, in turn, can help raise their offspring. Oberzaucher presents data from neuroscience to support the idea that men and women's brains are wired differently such that women are more emotional. On this she says the following:

> Modern techniques like fMRI (functional magnetic resonance imaging) and EEG (electroencephalogram) allow us to watch the brain while it is preoccupied with specific tasks. This research shows that female brains seem to be more emotional—that is, emotional activations are more intense and involve larger areas (Wildgruber, Pihan, Ackermann, Erb, & Grodd, 2002). Mapping such activation patterns onto task performance (for example, attributing the correct emotional state to a facial expression) allows us to understand the physiological aspects of sex differences in cognition (Baxter, Jackson, & Bain, 2003; Baxter, Sykin, et al., 2003). The development of the brain seems to be affected by testosterone (Knickmeyer et al., 2006), which modulates the differential growth of brain areas toward a masculinization of the brain. The human brain shows plasticity throughout one's lifespan, and hormonal environments continue to affect its functioning. A dramatic change in testosterone and estradiol levels in adult life can modify the physiology and cognitive processing, such that testosterone shifts the processes toward a masculinized brain and estradiol toward a feminized brain, in terms of distribution of activations (Schoning et al., 2007; Sommer et al., 2007). (Oberzaucher 2013, 356)

There are serious problems with Oberzaucher citing five out of the six articles she does to support the claims she is making. Starting with the first reference (Wildgruber et al. 2002),

Oberzaucher implies the reference supports the possibility that "emotional activations are more intense and involve larger areas" in women's brains than in men's. This particular interpretation of the findings is, however, quite mistaken. The article is in fact not looking at "emotional activations" in participants. It is investigating where in the brain acoustic emotional intonation in human voice is processed. Participants in the study, while being scanned in an fMRI, listened to a recording of a woman actor who was trained and instructed to speak four sentences in three different emotional tones. The only sex difference discussed in the study pertained to where in the brain men versus women process emotional speech. The authors suggest their findings support the possibility that men have more "prominent lateralization of language representation" than women (p. 867). Mind you, they only suggest this cautiously as other similar studies do not corroborate this. There is no discussion of whether or to what extent the activations observed are "emotional" activations. There is also no actual discussion of the significance of the activations observed in women's brains taking place over a "larger area." While the numerical data do indicate that this is the case, the authors do not draw attention to it nor do they suggest that such a result could translate into women's brains being "more emotional."

Oberzaucher's choice to cite the two articles by Baxter and colleagues in support of the claim that mapping activation patterns onto task performance helps us understand the physiological aspects of sex differences in cognition is also a bit confusing. The first article (Baxter, Jackson et al. 2003) is not a neuroimaging study. It's a questionnaire study looking at how an interviewer's interviewing style can interact with an interviewee's self-esteem. The paper does not even discuss sex differences in any way. It seems to have been cited by mistake. Why she cited the second study (Baxter, Seykin et al. 2003) is also unclear. While it is an fMRI study, and does look at sex differences in cognition, it does not make any claim regarding the

significance of neuroimaging data for understanding the physiological aspects of sex differences in cognition. In fact, at one point the authors of the study state,

> Further investigation is needed to understand the mechanism underlying the sex difference observed on fMRI studies. Presumably future studies will determine if these results represent a sexual dimorphism of structures associated with language ..., possible factors associated with circulating estrogen..., or other, as yet unknown, mechanisms (270).

In other words, the authors seem to be saying the opposite of what Oberzuacher cites them to say. Far from claiming that their fMRI study sheds any light on the physiological aspects of sex differences in cognition, they are emphasizing that more research, and more research from outside fMRI studies, is still needed before any conclusions about the mechanisms underlying sex differences found in fMRI can be determined.

Regarding the final two articles Oberzaucher cites, the most important thing to note is that neither of the articles engages the highly gendered language Oberzaucher uses in her final claim. Oberzaucher cites them to support the claim that testosterone in adult life shifts cognitive processes toward a "masculinized brain" and estradiol toward a "feminized brain." While both articles do discuss the effects of sex hormones on activation patterns, neither goes so far as to suggest such patterns reflect "masculinized" and "feminized" brains. This might be especially because the articles are each only looking at the effects of sex hormones on very specific types of brain function (i.e., mental rotation and verbal fluency). Even if the studies found testosterone and estradiol levels affect activation patterns in robust ways (which neither study really did), conclusions about men's and women's brains *tout court* would be highly inappropriate. Again, then, it is unclear why Oberzaucher has cited these particular articles for the reason she does.

There are other instances in which FEP scholars make claims about the natural role of women as nurturers in the absence of any data at all. For instance, see these excerpts:

> Before there was any division of labor and specialization sufficient for anyone to qualify as a professional anything, mothers telling stories to their descendants, and telling those descendants to repeat the stories to their own descendants, was crucial to human existence. (Coe and Palmer 2013, 124)

> Why would the Soviet Union use women [for combat in World War II] more than other countries, and why would women so readily engage in combat? The answer, we believe, is in the location of the conflict. The forces of the United States, Canada, England, Japan, and Germany were predominantly fighting in foreign countries whereas in the Soviet Union, the conflict was taking place on Soviet soil. Soviet women were fighting in defense of their homes and families, and consequently were motivated differently in comparison, for example, to a 21-year-old American soldier heading off to fight in Europe. We propose that women tend to shun military service when they are asked to leave their homes and families to fight for political and abstract nationalistic purposes. (Meredith 2013, 376)

In both excerpts above, claims about women's nurturance are made in the absence of evidence. In the first, Coe and Palmer posit that women have been the primary storytellers to children since ancestral times. But this is an unsupported claim. Coe and Palmer do not cite any anthropological data that could indicate this, nor do they provide any argument as to why it is unlikely that men could have also told children stories. In the second excerpt, Meredith also fails to provide any real evidence to support her claim that "women tend to shun military service when they are asked

to leave their homes and families to fight for political and abstract nationalistic purposes." Women were simply not allowed to take on combative roles in World War II in the countries listed. The Soviet Union was unique in this respect. As such, Meredith cannot know one way or another if women would have served in combat for these other countries. Additionally, Meredith has done no work to rule out the possibility that, if given the choice, men might also be more motivated to fight for homes and families, in which case such a motivation would not be sex-specific.

Taken together, there are numerous striking instances in FEP research in which biologically-deterministic claims about women's nurturance are left inadequately supported. I argue social values surrounding women's nurturance could be partly responsible for this. In the instances where FEP scholars, i.e. Cameron and Garcia (2013) and Oberzaucher (2013), cite inappropriate studies to support their claims, it is possible that social values are influencing what they see in these studies such that they see support where there isn't any. In the instances where FEP scholars, i.e. Coe and Palmer (2013) and Meredith (2013), do not cite any studies to support their claims, it is possible that social values are helping them see their claims as "intuitive" and not in need of any evidence. While both of these possibilities are certainly only speculative (I cannot assess FEP scholars' beliefs), my main intention with this discussion is to raise the possibility that values are playing a role. I think this is a more responsible move than passing off the instances just discussed as mere mistakes. As I argue in Chapter 3, calling attention to mere mistakes in research does not address the possible negative role values might be playing. This would allow the values to remain unchecked and to perhaps influence future research. By drawing attention to them, even if it is speculative, I provide an opportunity for FEP scholars to control for these values in future research.

6.3.2 Women's Natural Dependence on Men

In this section, I highlight research in FEP that is meant to explore how and why women are competitive. I show, however, that values of a patriarchal Social Order may be guiding this research as well. FEP scholarship on women and competition focuses almost exclusively on how women compete over (wealthy) men. This focus on women as competitive only over men suggests an assumption that women are dependent on men. Whereas men are expected to compete over resources and social status, women are only expected to compete over *men* who are then assumed to obtain wealth and status *for* them. These assumptions are challenged, however, by the actual ubiquity of women's competitions outside domestic contexts.

In her research on women's competition, Fisher (2013) makes the unsupported claim that women primarily compete over men. She defines competition as, "when two or more individuals are in pursuit of the same resource and that resource is perceived to be insufficient in quantity." And with regard to women's competition she says, "I propose that the scarce resource is 'good men'" (21). By "good men," Fisher means men who either (a) "are good providers and possess the physical ability to serve as protectors, or (b) have "high gene quality" (24). She says women will compete over these types of men depending on whether they are seeking a long- or short-term mate respectively. Importantly, however, she proposes that women's competitions will only rarely ever be over short-term mates because such men are not in short supply:

> I should note that another possibility exists; women seeking a short-term relationship simply might not need to compete as much as women seeking a longer relationship. This prediction is based on the findings of Kenrick, Groth, Trost, and Sadalla (1993), who reported that, when considering women for relationships of various durations, men place

> more importance on female attractiveness as the expected duration increases. In other words, men have lower standards for a woman's attractiveness (which is an important element of male mate preference) when seeking a mate for a one-night stand, as opposed to steady dating or marriage. Subsequently, perhaps not the *form* but instead the *quantity* of competition changes in parallel to the length of the relationship. (24)

Piecing this together, then, Fisher's hypothesis surrounding female intrasexual competition is that women compete with each other primarily over men, and specifically over men who can provide for and protect them.

One salient problem with Fisher's paper, however, concerns the lack of evidence she actually presents to support her hypothesis. Throughout her article, she presents a variety of studies that purport to demonstrate *how* women compete over men, but these studies do not appear to make claims about whether women compete over men primarily. She cites one study that shows women in a single society in Zambia primarily compete over men, but Fisher notes that the women in the society live under extreme circumstances such that they are uniquely dependent on men for their livelihood. She also mistakenly cites Burbank's (1987) famous cross-cultural study of women's aggression to say that women in the cultures studied typically fight other women over men. However, in a footnote, Burbank (1987) herself makes clear that her research is strictly looking at what she calls "domestic aggression," which she defines as "female-initiated aggression that is found in the context of the home or neighborhood and is motivated by domestic concerns" (71). Thus, she says, women's aggressive behaviours "such as rioting or the torture of prisoners of war" were excluded from her study (71). This narrow focus on domestic aggression could be one reason why competitions over men make such a prominent appearance in Burbank's observations.

I suggest this focus on domestic types of aggression and competition is one reason why Fisher believes that the main resource women compete over is "good men." Absent from her discussion entirely is a consideration of women's competitions in professional contexts such as work or school. But why is this so? It certainly isn't the case that such competitions would be irrelevant from an evolutionary perspective. Women in such contexts are competing over resources such as wealth, status, experience, recognition, and opportunity. These are resources that could certainly have fitness consequences if acquired. It also isn't the case that only few women compete in such contexts. Women make up 40% of the world's work force,[22] and they are typically overrepresented in university enrollments in 32 out of the 34 countries of the Organisation for Economic Co-operation and Development (OECD) countries.[23] It seems very possible, then, that stereotypes about women's domesticity are influencing the focus of women's competitions over men in this research. If women are understood to be primarily domestic and dependent on men, it might explain the lack of research attention on women's professional activities.

Meredith's (2013) and Liesen's (2013) research also focuses on women's competitive behaviours in domestic contexts. In Meredith's discussion of women's intrasexual competition she, like Fisher, explores how women compete over men. At one point she mentions that, "Other examples of women competing [other than competitions over men] are numerous" (380). Here she mentions women's competitions for dominance within peer groups, bargaining when shopping, competitions in sports, and competitions for access to educational resources for children. It is striking, though, that among these "numerous" examples, only one is from an

[22] As reported by The World Bank (http://data.worldbank.org/indicator/SL.TLF.TOTL.FE.ZS).

[23] As reported by the *Economist* (http://www.economist.com/blogs/dailychart/2011/09/female-graduation-rates).

explicitly professional context (i.e., competitions in sports). And, importantly, this is not because there is a dearth of studies on women's competitions in professional contexts (e.g., Beaudoin 2006; Gneezy et al. 2009; Mahmood and Hanafi 2013; Lallemand et al. 2008). There are plenty to be found, especially relating to sports. Thus, like Fisher, Meredith's lack of consideration of women's competitions in professional contexts requires explanation.

Liesen's (2013) research explicitly focuses on domestic contexts for the study of women's competition. She argues that theorists who suggest women are uninterested in competing over resources, like status, do so because they are engaging a male-biased understanding of competition. She suggests that if theorists re-define words like 'status-seeking' and 'politics,' then women's competitions should become more apparent. For example she writes,

> When one disconnects status from the socioeconomic measures, it helps to see that girls and women seek status too. (53)

She also says,

> …when politics is defined as the management of conflicts of interests, it is easy to see how women are engaged in political struggles every day, starting at a very young age" (58).

She then goes on to suggest that women have different types of dominance hierarchies than men. She proposes that girls and women "organize themselves in webs of peer groups in which there are high status individuals in the center and lower status members in the periphery" (56). She bases this on literature that suggests females in primate troops have non-linear dominance structures, and research on adolescent girls that suggests the most popular girl (the "queen bee")

is usually the "center of attention" (56-57). Thus, for Liesen, focusing on women in domestic, as opposed to "socioeconomic," contexts is necessary in order to better observe women competing.

Again, however, her theory, like Fisher's and Meredith's, is surprising. Given that most women work for a living and/or go to school, and both contexts can be highly competitive, it is not clear what is so mysterious about women's competition. Liesen goes so far as to suggest that words like 'politics' and 'status-seeking' need to be redefined in order for them to be applied to women. But is this necessary? Liesen gives almost no data to support this. Her suggestion to redefine status-seeking is given in response to Anne Campbell's research on women's competition. She summarizes Campbell (2002; 2004; 2006) to be arguing that "women compete for resources, but not status, by using indirect aggression to avoid injuries," and that women "have little to gain, if not more to lose, by engaging in risky, physical aggression for status" (Liesen 2013, 52). It is to this that Liesen offers her alternative definition of status-seeking:

> …Campbell's argument that women are indifferent to status is based on males' experiences of competing for status. When one disconnects status from the socioeconomic measures, it helps us to see that girls and women seek status too. (53)

But there are a couple problems with Liesen's response to Campbell. First, it is hard to know if Campbell really claims that "women are indifferent to status" since Liesen does not indicate where in the two books and one article she cites of Campbell's that this is said. It's worth pointing out too that this is at least not Campbell's (2004) position. In this article, Campbell is focused on *aggression*, and argues specifically that women are less interested than men in using physical forms of aggression to compete. She only briefly mentions women's competitions over status:

> men more than women should be concerned with publicly visible dominance rank (and threats to it through affronts and humiliation), since this has considerable implications for their mating strategy and is a trait that affects female choice. (23)

Notice, though, that this is a statement of sex differences. Campbell is not claiming that women are *indifferent* to status, just that they might be more so than men. Second, Liesen is unclear about what she is redefining 'political' and 'status-seeking' *from*, so it is hard to know just what it is about these terms that she thinks is so inapplicable to women's competitions. For instance, she does not explain what she means by "socioeconomic measures." Does she mean to say that it is rare that women compete for wealth, so it is better to think about women's status-seeking in other terms, e.g., reputation, popularity? But this is confusing since women directly compete for wealth all the time when they compete for jobs, promotions, or acceptances to university programs. Though perhaps Liesen would not consider these types of competitions "socioeconomic." It is hard to say.

She offers even less in the way of what she means by 'political.' She only states that politics needs to be redefined to mean "management of conflicts of interest" (58). But this is unsatisfactory. Given that women occupy most corners of the professional sphere, including leadership, management, and politics itself, Liesen needs to do more to explain what she thinks the canonical meaning of 'political' is and why such a term is inapplicable to women.

Subsequently, there seems to be room in Liesen's reasoning for the influence of values. Many of her claims about women's competitions support stereotypes of women as domestic and apolitical but are unsupported by data. And some of her interpretations of data present women to be more disconnected from political and economic contexts than they actually are. Like Fisher and Meredith, Liesen's conclusions are consistent with cultural presuppositions about women's

natural place in the domestic sphere. As with the other two scholars, prominent aspects of women's lives (e.g., work, school) are not addressed in this work.

6.3.3 Powerful *for a woman*

Values that see men as socially and physically superior to women also appear to be guiding FEP research. In this section, I highlight examples in which FEP scholars attempt to portray women as powerful, or at least equal with men, but instead end up reinforcing harmful stereotypes of women that support beliefs about women's inferiority to men. Hence, buried in these FEP scholars' praise of women lies the inadvertent message, "powerful *for a woman*.

> As a first example, recall the following quote by Liesen's from the previous section.
>
> …when politics is defined as the management of conflicts of interests, it is easy to see how women are engaged in political struggles every day, starting at a very young age" (Liesen 2013, 58).

What is noteworthy about this quote in the context of the present section is that the brand of 'political' that Liesen associates with women is rather unremarkable. Being able to "manage conflicts of interest" sounds a bit like settling quarrels among friends and family. This is a far cry from the types of politics that men are assumed capable of (i.e., all of the rest of politics): leadership, control of resources and territory, diplomacy, activism, political deliberation. And as I mentioned in the previous section, it is not clear why Liesen's alternative brand of political is better suited to women when women commonly and actively engage so many other types of politics. Hence, according to Liesen, women are only political like men when politics is divorced from leadership.

As a second example, Kathryn Coe and Craig Palmer (2013) try to empower women by referring to them as "empresses of the kitchen" (122). Recall, these scholars hypothesize that human traditions evolved primarily as the result of women's instructional activities, namely those regarding cooking and storey telling. In other words, they are claiming that women's place has been in the kitchen since the dawn of humanity but that this is a good thing. Because women were exceptional at teaching crucial activities such as cooking, they could ensure such skills were passed on. Thus, Coe and Palmer attribute the evolution of traditions to women.

I argue, however, that it in fact does *not* empower women to tell them that they are the empresses of the kitchen when, for most of history, women have been forced to cook and clean for their families without pay or rights. Add to this the fact that, even today, women are highly underrepresented in the restaurant industry and face severe forms of discrimination that thwart their breaking into the top tiers of culinary practice (e.g., see Bielski 2015; Jackson 2011). For while women's "place is in the kitchen," it certainly isn't in the commercial kitchen.

My third example comes from Wilbur and Campbell's (2013) study on women's alleged evolutionary preference for humorous men. The language the authors use to describe what they see to be the importance of this preference expresses admiration; women are said to hve a "powerful" influence in the mating arena because of the preference: The females of several species choose their reproductive mates, based on assessing reliable cues of mate quality. Human females appear to be no different, typically seeking evidence of long-term commitment, but strategically adjusting their preferences and their behavior when obtaining mates of high genetic quality becomes paramount. In contrast to how they are commonly depicted in classic fairy tales and contemporary media, women clearly exert a powerful influence in the realm of

> reproduction, a realm at the core of the evolutionary process. (Wilbur and Campbell 2013, 341)

So, according to Wilbur and Campbell, it is praiseworthy that women prefer to date funny men because it shows that women have created sites of selection among men that have led to cognitive advancements in humans associated with humour. But it is quite difficult to see how this is a compliment to women. There is nothing particularly impressive about someone who has the mere capacity to decide that a witty, entertaining person would make a good mate. On the contrary, the person who has the capacity to charm and entertain someone seems far more active, intelligent. Of course, men and women have both capacities. But this gets lost in Wilbur and Campbell's portrayal of humour as a male strategy, and choice of a humorous mate as a female strategy.

Newson and Richerson (2013) give an example of women's economic independence, but also end up harmfully stereotyping women. In their paper, they use their theory of "flexible parenting" to explain why cultural events such as economic change can bring about changes in attitudes about gender roles. A large part of their project is to argue against the idea that parenting roles are genetically determined. It is unfortunate, however, that the one example they provide to demonstrate how economic change can lead to women's economic independence is one that depicts women gaining the freedom to work only so that they can "buy fashionable clothing":

> As a population undergoes economic development its members increasingly identify with social groups other than their families and are influenced mostly by people who have no interest in their reproductive success. Within families, the importance of marriage and motherhood may still be discussed. But at work, in school, in the streets of towns, and in

> the media, other options get talked about. For example, women may learn about the benefits of getting a job and earning money to buy fashionable clothes rather than helping at home or earning money to help their family. Reproductive norms do not change immediately but they gradually diverge from that which encourages reproductive success. (Newson and Richerson 2013, 162)

The authors here seem guided by infantilized beliefs about women. Out of all of the things that women might do with their economic freedom (e.g., open a business, buy a house, go to university), the authors' example of choice is a trip to the mall.

So far, the above examples demonstrate that the assumptions in this FEP scholarship are consistent with the basic definition of patriarchy that men, not women, hold positions of power and authority. Even when women are to be praised for their "power," the instances in women's behaviours chosen are decidedly not powerful. Dressing up these behaviours as powerful succeeds at little more than sending the message that such behaviours are powerful *for women*. It would be hard to imagine very many men feeling a sense of pride if they were told that their greatness resided in their skills in forced, unpaid and underappreciated domestic work, their capacities to *appreciate* witty people, their abilities to manage conflicts of interest, or their independent choices to buy "fashionable clothing." But these are stereotypes of women, and, specifically, stereotypes that characterize women as unintelligent and unsuited for social leadership. They are not empirically-informed assumptions.

In one last example, beliefs about women's social but also physical inferiorities surface in Sylwester and Pawlowski (2011). In their evolutionary account of risk taking in men and women, women's risk taking is presented to be both physically and morally inferior to men's. Sylwester

and Pawlowski start out hypothesizing that risk-taking behaviour is an attractive signal in both men and women because it signals good genes:

> The focus of this study was risk taking as a consequence of intersexual selection. By voluntarily engaging in risky activities individuals are able to advertise their physical fitness, health, status, resources and desirable personality traits to peers and potential mates. At an evolutionary level, risk taking can be a signal of "good genes", that is genes promoting offspring's survival and reproductive success. (696)

As the article goes on, however, this hypothesis gets confused. It becomes apparent that risk-taking behaviour, according to Sylwester and Pawlowski's account, does not actually signal good genes in women, but rather promiscuity. Consider these quotes on the next page. Regarding how women interpret risk taking in men, they say,

> Short-term [male] partners are not likely to become long-term resource providers, but if they have good genes, these will be inherited by the women's offspring who will be more attractive to future sexual partners (Weatherhead and Robertson 1979). Hence, if risk taking can be seen as a signal of good genes in mate choice, women who pursue a short-term strategy should be sensitive to this cue and should favour risk-taking men. (697)

However, contrast this to what they say about how men interpret risk taking in women.

> From an evolutionary perspective, men's preference for risk-taking women as casual partners may be dictated by smaller mating effort necessary to court a mate. Women, usually more sexually restricted than men (Schmitt 2005), may demand long and costly courtship. A woman who exhibits a liberal approach to social norms or relaxes her social behaviour by consuming alcohol [a risky behaviour] could be an easier and less costly target in terms of sexual availability for men pursuing short-term relationships. (697)

Sylwester and Pawlowski do not explain this switch. And, importantly, the switch does not make a lot of sense according to the good genes hypothesis in evolutionary biology. If risk taking is a signal for good genes, then men will benefit from being attracted to female risk takers *because of* their good genes. For instance, some reproductive benefits that would come along with being attracted to someone with good genes include, having more offspring (because your partner is healthy and/or has physical abilities to secure sufficient resources), having healthy offspring (because your partner passes on their healthy genes or is a good caretaker), or having offspring who themselves have high reproductive success (because they are healthy, or can acquire resources, or can attract mates). It is not clear where promiscuity fits in here. Do Sylwester and Pawlowsky mean to say that risk taking is a signal for promiscuity which is, in turn, a signal for good genes? If so, this would need to be spelled out. Otherwise, if Sylwester and Pawlowski want to say that men have evolved abilities to detect women's promiscuity through women's risk taking, then this would have to be a separate hypothesis.

To explain this mistake, patriarchal gender beliefs might be doing some work here. The traits Sylwester and Pawlowksi attribute to good genes include, "physical fitness, health, status, resources and desirable personality traits to peers and potential mates" (696). Patriarchal stereotypes would more readily attribute most of these kinds of traits to men given that the traits are indicators of leadership. In line with these stereotypes, these traits are readily applied to men in the article but not to women. Almost the only type of risk taking behaviour attributed to women is substance abuse. Only when they are citing secondary studies on risk taking in women do Sylwester and Pawlowski briefly mention other types (e.g., two different articles cited mention aggression and sports). This is curious indeed for, on the face of it, these other types of risk taking fit better (than binge drinking) with the hypothesis that risk taking signals good genes.

The language that is used to describe male versus female risk takers also fits gender stereotypes in that women's risk taking is presented as morally inferior to men's. On a few occasions, the authors borrow gendered language from the articles they cite to characterize male risk takers as "heroic," "daredevils," or "brave," but they do not use this language for women:

> Kelly and Dunbar (2001) looked at female preferences for altruistic versus nonaltruistic bravery in men. The results…indicated that women preferred daredevils who took risks without the altruistic component as short-term partners over risk-averse men and men who took heroic risks. (697)

Also,

> Our results…support Kelly and Dunbar's finding (2001) that for women, non-heroic physical risk taking is a favourable trait in short- but not in long-term partners. (702)

Finally,

> Our finding of female preferences for risk taking is consistent with the notion of "cad" and "dad" mating strategies (Belsky et al. 1991). A number of studies reported that for short-term sexual liaisons women desire competitive, dominant and brave cads who can provide them with genetic contribution likely to result in sons of similar characteristics (Gangestad and Simpson 2000). (702-703)

On only one occasion is such language used to describe physical risk taking in women:

> Men's choices deviate from our prediction in the sense that, although financially and socially risk-avoiding partners were frequently chosen, so was the physical risk taker. Like women, men might seek long-term mates who are *brave* enough to take risks in order to protect their offspring. (emphasis added 704)

All other descriptions of women's risk taking, however, pertain to alcohol consumption and do not implicate any admiration. In one instance, risk taking in women is even described in morally pejorative terms:

> It pays for men to court a specific type of women for a short-term relationship. First, if men find a mate who easily agrees to a casual relationship they reduce their mating effort. Women under the influence of alcohol and drugs (physical risk takers) were shown to engage in short-term liaisons more often than sober women (Grello et al. 2006). In another study, the amount of money spent on alcohol was a good predictor of unrestrictiveness in sexual behaviour (Clark 2004). Social risk taking, as we defined it, involves immoral behaviour that is in conflict with commonly accepted norms. Unrestricted female sexual behaviour easily falls into this definition. Again, if a man chooses an unrestricted woman as a short-term partner he will reduce the time and energy spent on courtship. The results obtained from the second task also support our hypothesis with almost 70% of men choosing either physical or social risk taker as a short-term partner. (Sylwester and Pawlowski 2011, 703)

Accordingly, to Sylwester and Pawlowski, physical risk taking in women amounts to binge drinking which leads to "unrestricted female sexual behaviour," which is "immoral." This is a far cry from "daredevil" or "brave." I should point out as well that Sylwester and Pawlowski *did not* define social risk taking in the way they claim in this passage. In particular, the word 'immoral' was not used in the definition. The definition they provided is as follows:

> We defined social risk taking as any behaviour which can lead to a decrease in reputation, ostracism or social punishment. In some circumstances, social risk taking can also signal

dominance, as only individuals of high status can afford to behave in an uncompromising way. (699)

This definition of social risk taking does not require the experimenter to make a moral judgment about the behaviour in question. It only requires them to make a judgment about whether the behaviour violates social norms or is dominant. It also isn't the case that all behaviours that lead to "decrease in reputation, ostracism or social punishment" are necessarily immoral, but this is what the later alternative definition of social risk taking that they attribute to women's binge drinking implies. Hence, whatever the authors' reason is for doing so, it is gratuitous for them to make a moral judgment about women's sexual behaviour. (Note as well that social risk taking as a signal of "dominance" drops out of the definition when Sylwester and Pawlowski define it in the context of women's behaviour.)

Taken together, despite Sylwester and Pawlowski's proposition to discuss why and how risk taking is a signal for good genes in both men and women, they end up only doing so for men. For reasons unknown, the authors only discuss the significance of binge drinking in women, and even in this regard do not attribute the behaviour to good genes but rather to a seemingly unrelated quality, promiscuity. Risk taking behaviours are described differently in men and women. Men who take risks are "brave," "daredevils," and "heroic," whereas women are "less costly," "immoral," and "unrestricted." Beliefs about women's social physical inferiorities might be preventing Sylwester and Pawlowski from seeing women as possessors of the kinds of qualities associated with "good genes," or capable of taking physical risks beyond alcohol consumption.

As my analysis in this second part of the chapter demonstrates, FEP research portrays women primarily as caretakers and dependent on men. FEP scholars also portray women as

socially and physically inferior to men when they attempt to praise women for decidedly unimpressive leadership or social attributes or unremarkable risk-taking behaviour. Based on my analysis, I argue that (1) patriarchal values of Social Order undermine the quality of FEP research, and (2) jeopardize FEP scholars' feminist aim to contribute to resolving social issues of gender inequality. Regarding my first argument, my analysis demonstrates that patriarchal values bias in problematic ways what FEP scholars choose to research (e.g., women in limited contexts like domestic contexts), what they see in their own as well as others' data (e.g., confirmation of gender stereotypes that isn't actually there), and their choice of language (e.g., morally-charged language in characterizing men's and women's risk taking). Regarding my second argument, the implicit and explicit claims about women in FEP scholarship reinforce beliefs that women are most suited for caring and domestic roles and unsuited for leadership. It is hard to see how these kinds of claims could generate any kind of positive social impact especially in the way of helping eliminate gender inequality.

I recommend FEP scholars put structures in place that help them scrutinize their values. I give three structures that could help them do so here. First, FEP scholars could engage a wider range of feminist literature and scholars. The FEP scholars in my literature sample cite few feminists. More integration into other feminist communities would help broaden their feminist perspectives and help them better understand the complexities of women's issues and how scientific research can be harmful to women.

Second, I suggest FEP scholars specifically engage feminist scholarship on values in science. This scholarship explores the different ways prejudicial values can influence a scientist's research, the real harms of such values, and what good replacement values are (e.g., see Bleier 1984; Dotson 2011; Fehr 2004; 2011; Fricker 2007; Grasswick 2004; Haraway 1988;

Harding 1986; Melo-Martin and Intemann 2012; Longino 1990; 2002; 2006; Longino and Doell 1983; Tuana 1989). Reading this scholarship can help FEP scholars know where to look for harmful values in their own research and how to correct for them.

Third, as an exercise, FEP scholars could consider asking themselves a series of questions that I have put together that is designed to help one scrutinize harmful values of Social Order specifically. The series of questions would require FEP scholars to first consider if they themselves hold essentialist views about certain people groups. To hold an essentialist view about a group of people is to believe that there are sets of attributes that apply to all or most members of that group, and that these attributes determine membership in that group. The questions then guide FEP scholars to consider whether or how these essentialist beliefs make their way into their research. They then require FEP scholars to consider if these essentialist beliefs are warranted. If the beliefs are warranted, they must consider if the beliefs indicate a harmful Social Order. If so, FEP scholars should consider if the benefits of producing scientific scholarship that reinforces this Social Order are worth such harm. If they believe that they are, they should make this explicit in the research itself so that readers are aware of the harms, and can consider for themselves if accepting the research is worth it to them. Here are the questions:

Seven Steps for Scrutinizing Harmful Values of Social Order

(1) When I think of people group P, do I see a particular paradigm person, R?
(2) Does the language I use in my research give away that I mean R when I say P?
(3) If so, am I *warranted* to portray P as R, or is it important in any way that the people in P who are not R also be represented?
(4) If I am warranted, does my presentation of R indicate a harmful Social Order? For instance, do I associate R with qualities that are mainly inconsistent with social leadership?
(5) If R indicates a harmful Social Order, is it *necessary* for my research that I present P as R, or is there an alternate version of R (e.g., R'), that is also representative of P, that is not associated with qualities that are mainly inconsistent with social leadership?

(6) If there is no alternate version of R, is producing the research *worth* the harms of associating P with R?
(7) If the research is worth the harms, explain your reasoning to your readers.

I should, however, caution about an important limitation to this set of questions: the problem of implicit bias. Implicit biases are those biases that persist below our level of awareness. Psychological research on implicit bias teaches us that we are not always aware of our prejudices, or the kinds of qualities that we associate with certain people groups. Brownstein (2017) gives the following example:

> imagine Frank, who explicitly believes that women and men are equally suited for careers outside the home. Despite his explicitly egalitarian belief, Frank might nevertheless implicitly associate women with the home, and this implicit association might lead him to behave in any number of biased ways, from trusting feedback from female co-workers less to hiring equally qualified men over women. (n.p)

The problem with implicit bias, then, in regards to my set of questions, is that scholars who use the questions might unjustifiably answer "no" to questions 1 and 4 because they simply are not aware of their beliefs about the people groups they are studying. This would defeat the purpose of the questions. Because of this, the set of questions should only be used as a helpful exercise for now. Future social psychological research could test whether the questions are effective for investigating one's beliefs about the people groups they study, and perhaps help determine how the questions could be re-written so that implicit bias is minimized. Until then, I would emphasize that FEP scholars use the questions in conjunction with especially my first recommendation, that FEP scholars engage a wider range of feminist literature and scholars. Where a FEP researcher might hold an implicit bias about the people group they are studying, their feminist colleagues from outside FEP might be able to bring another perspective.

Conclusion

My analysis of FEP reveals that problematic values of Authority and Social Order guide FEP scholars' interpretative judgments of their data as well as their choices of study. As a result, such values compromise the quality of their research and thwart their stated feminist aims. I showed how FEP scholars' imperialist values of Authority may be playing a part in their mischaracterization and dismissal of feminist concerns about EP. I also showed that because FEP scholars buy into an untenable vision of scientific unity, and one that prioritizes EP explanations over non-EP explanations, they miscalculate how important it is that non-EP feminists incorporate EP into their accounts of human behaviour/psychology. With regards to FEP values of Social Order, I revealed the problematic ways in which FEP valuation of women's roles as nurturers and subordinates, and men's roles as leaders likely influenced FEP choices of study, focus, language use, and data interpretations. FEP scholars highlight the importance of nurturing behaviour in women at the expense of focusing on other types of behaviours. Values of the importance of caretaking in women appear to be filling in gaps between FEP conclusions and actual evidence provided, and even leading FEP scholars to see evidence for the importance of women's nurturance when it is not there (e.g., misunderstanding/misreading secondary literature). Values of men's leadership and women's dependence likely lead FEP scholars to highlight how women are only competitive in domestic contexts or over men. Values that see men as physically and socially superior to women lead FEP scholars to credit women with underwhelming examples of leadership, power, independence, or risk-taking tendencies. According to my analysis, then, eliminating these problematic values in FEP could contribute to better quality research and help FEP better achieve its feminist aims.

Chapter 7: Conclusion

In this dissertation, I developed the social-dimensional approach as a method for the philosophical critique of morally relevant science. I applied the social-dimensional approach to a new branch of evolutionary psychology called feminist evolutionary psychology (FEP). I showed that this new science has serious epistemic and ethical flaws. I addressed the implications of these flaws and offered recommendations for how feminist evolutionary psychologists can address them. In this final chapter, I summarize the contents of this dissertation and discuss future directions for philosophical work using the social-dimensional approach.

7.1 Dissertation Summary: Applying the Social-Dimensional Approach to FEP

In Chapter 3, I developed the social-dimensional approach from my analysis of the philosophy of biology literature on the criticism of evolutionary psychology (EP), the study of the evolution of human psychology and behaviour. From this literature, I teased out two different methods of scientific critique. The first I called the "truth-detectional" approach. Those who take this approach are first and foremost concerned about the truth of EP claims as that truth can be determined by evidence. The second I called the "social-dimensional" approach. Those who take this approach talk about the production and truth of EP claims but within a social framework. On this account, the legitimacy and perceived legitimacy of EP claims are not separate from the institutional and social processes and values that lend to their production. I showed that the truth-detectional approach risks harms to society and to the philosophy of science, but that the social-dimensional approach avoids these harms. Because of this, I argued that philosophers of science should take the social-dimensional approach to the assessment of morally-relevant science.

In Chapters 4 and 5, I applied the social-dimensional approach to feminist evolutionary psychology. I mentioned several reasons (see Chapter 1) regarding why FEP should be assessed using the social-dimensional approach. First, I explained that because feminist evolutionary psychologists still conduct their research broadly within the disciplinary framework of EP, their epistemic practices (e.g., theories, assumptions, data practices) may be flawed in similar ways that EP practices are. My assessment of FEP, using the social-dimensional approach confirmed that FEP epistemic practices (e.g., theories, assumptions, data practices) are flawed in similar ways that EP practices are. In Chapters 4 and 5, I found evidence that FEP scholars make many of the same mistakes in their epistemic practices as evolutionary psychologists have in the past, as well as new ones. Some of these old mistakes include, using an outdated and thoroughly criticized theoretical paradigm (the BTP paradigm), and not adhering to standards in evolutionary biology regarding the appropriate kinds of data needed for supporting adaptationist hypotheses. I developed the OEP check and discovered FEP scholars also fail to properly operationalize the phenomena they study for the purposes of adaptationist research.

Second, I pointed out that because feminist evolutionary psychologists still operate within an EP framework, the sexism embedded in EP might also be present in FEP. In EP research, men are often attributed qualities and roles that pertain to leadership and selfishness, and women qualities and roles that pertain to servitude and nurturance. These qualities are often claimed by evolutionary psychologists to be innate and hard to change. My assessment of FEP in this regard in Chapter 6 uncovered patriarchal values of Social Order. I found evidence that beliefs in FEP about men and women mirrored unjustified stereotypes of men and women. Without adequate evidence, women's nurturance was presented in FEP to be their predominant contribution to evolution. Without adequate evidence, women were also presented to be naturally dependent on

men, and socially, politically, and physically inferior to men. I showed how patriarchal values of Social Order in FEP were partly to blame for these false and harmful claims about women. I also uncovered imperialist values of Authority. I argued that FEP has problems with Authority because (a) there is a theme in FEP scholarship in which FEP scholars seek to advise feminists on how to conduct their research but lack the knowledge and expertise to do so, and (b) FEP scholars depend on an unjustified expectation of the unity of science to argue feminists should incorporate EP research into their work. I demonstrated that FEP's values of Social Order and Authority compromise the quality of FEP research and its feminist aims. Patriarchal values of Social Order bias in problematic ways what FEP scholars choose to research (e.g., women in limited contexts like domestic contexts), what they see in their own as well as others' data (e.g., confirmation of gender stereotypes that isn't actually there), and their choice of language (e.g., morally-charged language in characterizing men's and women's risk taking). Patriarchal values of Social Order in FEP research also lend to the production of claims about women that they are most suited for caring and domestic roles and unsuited for leadership. It is hard to see how these kinds of claims could generate any kind of positive social impact especially in the way of helping eliminate gender inequality. Imperialist values of Authority in FEP promote a homogenization of feminism in a way that would narrow feminist focus and limit the ways in which feminists could understand and solve social problems.

A third reason to assess FEP, as I pointed out in Chapter 1, is because the discipline has the potential to produce science that is both socially progressive and epistemically rigorous if its epistemic and ethical weaknesses are addressed. FEP scholars bring new perspectives, theories, and methods to the evolutionary study of humans in general and women in particular. They are also conscious of the ethical dimensions of their discipline as is evident in their efforts to apply a

feminist perspective to their studies of women. A critical assessment of FEP can therefore help further its development. Throughout Chapters 4, 5, and 6, I provided recommendations for how FEP scholars might improve their research by addressing the criticisms I've laid out. In Chapter 4, I provided FEP with alternative theories in evolutionary biology that are relevant to FEP research but not susceptible to the same problems that the BTP paradigm is. In Chapter 5, I made a case for the importance of properly operationalizing the phenomena one studies. I demonstrated, using examples from FEP scholarship, how this can be done poorly, and I offered suggestions for how FEP scholars might be more vigilant about their operationalizations. I also provided FEP scholars with references to literature in philosophy of biology that outlines the appropriate standards for evidence that FEP scholars need to support their adaptationist hypotheses. I also recommended better data collection and interpretive practices. Finally, In Chapter 6, I provided FEP with recommendations for how they can better scrutinize their values such that they can avoid the influence of the problematic and harmful values of Authority and Social Order that currently guide their research. I suggested ways they can be more pluralist in their feminist approach to the study of women (to address imperialist values of Authority), and offered pointers for how they might build in critical structures in their research that could help to catch unjustified essentialist beliefs about people groups (to address harmful values of Social Order).

7.2 Future Directions: Broader Application of the Social-Dimensional Approach

In this dissertation I demonstrated the applicability of the social-dimensional approach to evolutionary psychology (Chapter 3) and to a new branch of evolutionary psychology, feminist evolutionary psychology (Chapters 4, 5, & 6). In future work of mine I will apply the social-

dimensional approach to two morally relevant sciences outside EP, the neuroendocrinology of human paternal care and the epigenetics of sexual orientation. The neuroendocrinology of human paternal care answers questions about the relationship between caring behaviour in human fathers and underlying neuralhormones (e.g., testosterone, vassopressin, prolactin) and brain circuits. Past research on the neural correlates of parental care was heavily focused on women; recently, however, some neuroendocrinologists have seen the need to study the neural correlates of human fathering. Discussions in these papers focus explicitly on social justice issues such as gender equality in parenting (e.g., Gettler et al. 2011a) and how the exclusion of men from neuroendocrinological studies on parenting (e.g. postpartum depression) has a negative impact on men, their partners, and the children they raise (e.g., Swain et al. 2014). The researchers who study the epigenetics of sexual orientation consider whether and how epigenetic mechanisms (e.g., nucleosome repositioning, DNA methylation, and/or modification of histone tails) contribute to the development of specifically lesbian, gay, and bisexual (LGB) orientations. Researchers in this programme teach an awareness that "historical, social and religious norms have interfered with a full appreciation of the scope and diversity of the homosexual phenotype in nature" (Rice et al. 2012, 769). They point out how the epigenetics of sexual orientation better captures the rich variation that exists across LGB individuals because it highlights the complex interplay between genomic mechanisms and the environment (e.g., Ngun and Vilain 2014).

Despite the attention to morally relevant issues in these scientific domains, I expect that my social-dimensional analyses of them will uncover persistent and hidden prejudicial assumptions and values. For example, I have already found heteronormative assumptions in the epigenetics of human paternal care about the complementariness of a genetically male man and a genetically female woman for adequate parenting. I have also found instances of language use

that indicate prejudicial assumptions about men and women. For instance, in one study the authors (Gettler et al. 2011b) refer to women as the "females" that "humans" mate with (16194), casting men as paradigm humans and women as oppositional to this paradigm. Similarly, in the epigenetics of sexual orientation there are assumptions that LGB orientations are the result of "dysfunctions," and that those who identify as LGB can be treated as a homogenous class of people.

As a social-dimensional analysis of sciences like FEP, the neuroendocrinology of human paternal care, and the epigenetics of sexual orientation demonstrates, science is not separate from the social and moral. Scientific claims can have serious social impact, and social values can guide how research is conducted, interpreted, and assessed. Given their training and opportunity for engagement, philosophers of science are opportunely situated to assess the social and moral dimensions of science. My development and application of the social-dimensional approach in this dissertation provides a valuable tool for philosophers of science to adequately engage the social and moral dimensions of science. The social-dimensional approach is designed to assess specific social and moral dimensions a science (i.e., social values and impact) and to consider what role those dimensions play, or should play, in that science's production of knowledge. In regards to FEP, I showed how FEP's feminist ethical considerations of women have led to novel developments in evolutionary theory and the study of women (see Chapters 1 and 2). I also showed how harmful values of Social Order and Authority in FEP compromise the quality of their research. Future work of mine will demonstrate how the social-dimensional approach can be used to make similarly valuable assessments of the neuroendocrinology of human paternal care and the epigenetics of sexual orientation. I encourage other philosophers of science to further broaden the social-dimensional approach's applicability by applying it to the morally

relevant sciences they study. I also encourage philosophers to expand the approach itself. For instance, philosophers might want to add considerations of other types of social dimensions to their assessments such as, for example, considerations about how scientists' social positions or memberships in social groups (race, ethnicity, sexual orientation) affect how they produce knowledge (see Harding 1986; Wylie 2003 for discussions about how the situatedness of knowers matters in the production of knowledge). As I argued in Chapter 3, developing and applying philosophical tools like the social-dimensional approach provides an important opportunity for philosophers to contribute to the production of socially responsible science.

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